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(Mr. Carmichael's Paper.)

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NOTICE TO CORRESPONDENTS.

The Review of Dr. Cowan's important Paper on the Statistics of Fever and Small-pox was received too late for insertion in the usual place; but it will be found under the head of "*Notices of Works received.*"

Dr. Bardsley's Retrospective Address, and Dr. Cormack's Thesis, have been received.

ERRATA.

(Mr. Phillips' Paper, No. 34.)

Page 129, line 11, *for on read in.*

— 122, — 33, *for of which, read which.*

— 124, — 38, *for sulphurated, read sulphuretted.*

— 124, — 39, *for on read in.*

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1 SEPTEMBER, 1837.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Observations on Bruit de Soufflet.* By DAVID C. NAGLE, A. B., M. B., one of the Physicians to the Dublin General Dispensary.

“**E**VERY man,” said the good and philosophic Johnson, “from the highest to the lowest condition, ought to warm his heart and animate his endeavours with the hopes of being useful to the world by advancing the art which it is his lot to exercise.” This principle, thus so nobly inculcated, seems so much to influence the zeal of some distinguished physicians, by whom this—the metropolis of my native country—is, at present, so brilliantly ornamented, that one is led to expect from it, if well directed, results the most beneficial to the interests of medical science. And I feel that those interests will not be compromised by an attempt to check this laudable spirit of inquiry, whenever it becomes either too rapid in its career, or likely to deviate too much into the regions of fancy.

Influenced by feelings of this description, I hope it will not be unproductive of advantage to direct attention to a paper on

“the Mechanism of *Bruit de Soufflet*,” which appeared in the number of this Journal for November last. It will, at least, be excusable, as in a paper, published in the *Lancet* for May 28th, 1831, in opposition to the doctrine then recently propounded, regarding the “motions and sounds of the heart,” I expressed myself to the following effect:—“I confess I cannot possibly bring myself to subscribe to the doctrine of those, who would persuade themselves and others, that *bruit de soufflet* is owing to the passage of the blood through a narrow opening into a wider cavity, and detected only in the latter.” Independently of some facts there adduced in support of my opinion, I added, “besides their principle will not account for the *soufflet* heard during pregnancy.” The author of the theory having lately more fully developed his ideas on the mechanism of *bruit de soufflet*, and having adduced this uterine murmur in support of his views, it may not be useless to discuss the question; and consider whether his doctrine is, as he imagines, sufficient to account for the phenomenon “under all the apparently contradictory circumstances in which it is found to occur.”

“The two conditions,” says the author, “which constitute the mechanism of *bruit de soufflet* are; 1st, a current-like motion of the blood, tending to produce vibrations in the sides of the cavities or arteries through which it is moving; and 2ndly, a diminished tension of the parietes themselves, in consequence of which these parietes are easily thrown into vibrations by the irregular currents of the contained fluid, which vibrations cause, on the sense of touch, *fremissement*, and on the sense of hearing, *bruit de soufflet*.” In the middle of his paper he says, “We now come to one of the most singular instances of *bruit de soufflet*, that heard in the pregnant state:” and, from a work not long since published on obstetric auscultation, adduces the following extract to bear him out in his views. “The placental portion appears to consist of a congeries of vessels, tortuous, ramifying, and expanding into cells or sinuses, while the remaining part exhibits the parenchymatous structure of this

organ, with merely occasional vessels interposed." "There is therefore," he goes on to reason, "especially in the placental portion of the uterus, a stream of blood rushing from one or two arteries into an extensive system of enlarged tubes. After each contraction of the heart, the pressure of the contents of the uterus from within, and the counteracting pressure of the abdominal muscles from without, empty in some degree, or render flaccid the enlarged and tortuous arteries, either by pressing forward into the sinuses the blood which had just been driven into the arteries, or by throwing back a portion of it towards the main trunks, or probably by both. The tortuous arteries of the placental portion of the uterus, so much enlarged, compared with the trunk that supplies them, are in the intervals between the contractions of the heart, thus brought to the first condition necessary to the mechanism of this sound, and in this state receive into them the next dash of blood from the supply trunks, and this fresh portion of blood, propelled onwards, runs along in so many rushing currents through the tortuous arteries, throwing the sides of those vessels into corresponding vibrations, and thus giving to the ear *bruit de soufflet*"!

By the above extract, with which, as well as his reasoning on it, I would chiefly deal at present, it will appear the author assumes that the uterine murmur is seated in *that* part of the uterus to which the placenta is attached. I have elsewhere* endeavoured to show that the murmur is not *necessarily* seated in that part of the uterus; and until I find my views overturned by more judicious, legitimate, and convincing arguments, than are contained in the work from which he derives his opinion, I shall not be induced to relinquish mine. The site of the murmur is a point to which I would request particular attention, as being very important in the present discussion. Now, if the site of the uterine murmur be in *that* part of the uterus to which the placenta is attached, every intelligent and reasoning physi-

* See *Lancet*, Dec. 18th, 1830, p. 395, *et seq.*

cian will concede that the seat of the murmur *should vary* with the varying situation of the placenta ; and we are all aware that the latter is liable to be attached to the uterus anteriorly or posteriorly ; laterally, or over the os uteri ; but every man of experience and accuracy of observation will allow that the murmur, when it at all exists, is *invariably* heard in the *lateral* parts of the uterus along the course of the lateral uterine arteries ; and it is there, and no where else, that we should look for it in our examinations. Supposing the placenta to be attached over the os uteri, where should the murmur then be most audibly heard ? Unquestionably over the pubes. Can such a fact be found ? I think it cannot. After the most anxious attention to this question, I never yet could discover the *principal site* of the murmur in any other part of the abdomen than the lateral regions, as I have already described.* And, indeed, by manual examination, I have found the placenta to be attached to the upper part of the fundus of the uterus, while the murmur was heard in the usual situation. I therefore feel myself justified in denying the supposition, that the uterine souffle is seated in the arteries running through the placenta ; and consequently, that its mechanism in the uterus at least depends on the cause assigned. But let me, for argument sake, grant that the souffle is situated in these arteries : how then could we, according to the theory of “ vibration and current-like motion,” satisfactorily account for the astonishingly increased loudness which the souffle occasionally exhibits ; or for the vast and interesting variety in the *quality* of the murmur, which an acute and practised ear will, at times, easily detect during the state of pregnancy. From the spongy nature of the placenta, we know that it must necessarily be a bad conductor of sound ; and, consequently, that there cannot be any very exaggerated augmentation of the sounds produced by vibration, as there is in hepatization of the lung for instance : and if any one be so fanciful as

* See *Lancet*, Dec. 18th, 1830, p. 396-7.

to imagine that *vibration* in the arteries of the placenta—particularly when the circulation is rather languid—could, by possibility, produce the *loud* sound we often detect there, I may much admire such a rare endowment, however unwilling I may feel to be possessed of it. But let us analyze the above extract.

Pressure from within or without, I would respectfully submit, has nothing to do with the production of the murmur in question. How do I show it? Let the patient be turned on either side, and the only pressure which can be made—that by the foetus or liquor amnii—on the opposite one be thus lessened by gravitation, and let the abdominal muscles at the same time be relaxed; still we have the bruit de soufflet in all its intensity. Will any one deny this position?—The conclusion is inevitable. But let us suppose that pressure from within and without does empty the vessels running through the placenta in the manner described by the author; and let us pay some attention to the inferences he draws from such a supposition, and to his mode of reasoning on it, and we shall find it, I apprehend, rather weak, inconclusive, and *inconsistent*. “*After each contraction of the heart,*” says the author, “the pressure of the contents of the uterus from within, and the counteracting pressure of the abdominal muscles from without, empty in some degree, or render flaccid, the enlarged or tortuous arteries”!! If the pressure, thus imagined, be so great as to empty the vessels in the manner described, it follows, as a necessary consequence, that these arteries, kept so compact, so wedged between the compressing forces from within and without, cannot, by any possibility, be thrown into the vibrations, which the author deems requisite for the production of the sound in question, until that pressure is removed, and the vessels allowed their full play. And I cannot conceive by what magic it is he fancies the compressing force to be laid on *during* the INTERVALS *only* between each contraction of the heart, and then to be so accommodately removed at the very moment of contraction, for the admission of

“a fresh dash of blood.” One would suppose that the author was arguing in favour of the generally received doctrine regarding the cause of bruit de soufflet, by thus opposing to the blood’s free transmission a barrier, which must be burst through by the “next dash from the supply trunks,” before we can have the vessels in his fancied mechanical condition.

If this *ad absurdum* argument be not sufficient to point out the fallacious nature of his reasoning, let me request attention to the following incontrovertible fact, and the inferences which naturally flow from it. The uterine murmur is frequently perfectly *continuous*.* This cannot be denied. Let me then put it to the dispassionate judgment and candour of the author, whether he really thinks such a phenomenon possible, if his reasoning be correct. His position is: that the vessels in which he supposes the souffle to exist, must have the power of vibration—must be in a comparatively flaccid state to receive the “next dash of blood;” and yet he states that they must be, during the intervals between the heart’s contractions, so compressed, as to “force forward into the sinuses the blood driven into them, or throw back a portion of it towards the main trunks”! By such reasoning it appears—to say nothing of the many other incongruities which it involves—that the vessels must be, at the same moment, wedged between two compressing forces, and yet so flaccid, as easily to admit into them the “fresh dash of blood from the supply trunks.” Thus he evidently assumes that the murmur cannot be *continuous*; that is, does not occasionally exist during the *whole interval* between the heart’s contractions. This would indeed be a great mistake; and a little more practical acquaintance with the murmur will easily disabuse the intelligent author of such an erroneous opinion, and induce him to exert his praiseworthy zeal for the attainment of a theory of the phenomenon in question more probable, more likely to meet with the assent of the judicious and

* See Lancet, Jan. 8th, 1831, p. 800.

reflecting portion of the profession. It is a principle in dialectics, as well as in the art of war, to secure for one's self a safe basis for operations. But I greatly apprehend that the author has deviated from such a principle, by taking, as the basis of his reasoning on this point, the statements of a gentleman, who has, I fear, unfortunately involved himself too deeply in a wilderness of contradiction, to be prudently produced as a safe guide for determining the true site of the uterine bruit de soufflet.

Thus have I endeavoured to show the inconclusiveness of the author's reasoning, by,—1st, the unvarying situation of the uterine souffle; 2nd, its intensity, and, frequently, continuous nature; 3rd, the utter impossibility of internal and external pressure being the cause of it; 4th, the evident inconsecutive-ness, if you will allow me, of the reasoning on even such a supposition; 5th, the vacillating and self-contradictory statements of the gentleman from whose work an extract has been produced to establish the site of the murmur. And I am unavoidably forced into the conviction that the author has not been happy in adducing the uterine souffle in sustainment of his views on the mechanism of the bellows' sound. Is he more successful in his reasoning on its mechanism, as applied to the arterial system generally? That shall be the subject of a subsequent paper; and in treating of it—whilst I am ready to admit the utility of theoretical reasoning, if legitimately conducted—I at least shall keep before my view, what I am deeply persuaded of, that refined speculation too often becomes, like the “dream of the sleeper,” a false and empty dream.

ART. II. — *Cases of Inversion of the Uterus, with Remarks.*

By THOMAS RADFORD, Surgeon Extraordinary to the Manchester Lying-in Hospital, &c., and Lecturer on Midwifery at the Royal School of Medicine and Surgery.

INVERSION of the uterus differs in degree, and the terms partial and complete have been applied to characterize them. In par-

tial inversion the fundus may be more or less depressed, from a slight dimpling, to a protrusion through the os uteri into the vagina, or through the os externum. Under this division the writer classes all cases where any portion of the cervix remains uninflected above the os uteri, however short the angle of inflection may be. In complete inversion the cervix is not inflected upon itself, or embraced by the os uteri; but this orifice is effaced, and the vagina is inverted to a greater or less extent, the fundus projecting beyond the os externum very considerably.

Inversion has been denominated *active* or *passive*, more perhaps from the lapse of time, than from the particular condition of the organ. The writer suggests, for the consideration of the profession, whether the words *reducible* and *irreducible* are not more proper. This accident may be called *reducible*, however long it has existed, provided the practitioner can by any safe means accomplish its replacement; and *irreducible* when every effort has been made, but unsuccessfully. By adopting this nomenclature the practitioner is imperatively called upon to exercise fully the powers of his art, before he can classify the disease.

It is happily one of those misfortunes which does not so very often take place; although the writer is inclined to think much more frequently than is usually admitted.* The reason of this concealment is clear, if it is considered what opinion is entertained of the practitioner who has been so unfortunate as to meet with such an event; there is no occurrence which throws more odium upon him, although generally very undeserved. This feeling paralyzes the energies of the surgeon, and concealment is adopted to shield himself from this unjust imputation.

* Dr. King is of the same opinion. Mr. Mackenzie found two or three instances in subjects brought into the dissecting-room. Mr. Windsor, Med. Chir. Trans. vol. ii. Doctor Ramsbotham, Practical Observations, Part I. page 137.

Most writers ascribe the accident to rude pulling at the funis, and to violence in removing the placenta. This opinion, as well as others connected with this subject, equally important, will be found to be erroneous, if published cases are carefully investigated. The nature of this accident is extremely alarming; the patient either dies suddenly if it is not replaced, or gradually sinks, after a life of protracted misery, by the irritation and profuse mucous, purulent, and sanguineous discharge which takes place. The irreducible inverted uterus has been removed six times in the course of a few years in Great Britain; but the opinion of the writer is, that the operation of extirpation, sometimes fatal, always dangerous, and which does not in every instance relieve the patient from those discharges which induced her to submit to it, will become altogether unnecessary if early, judicious, and persevering attempts be made to reduce it. Great odium is deserved by the practitioner who allows a reducible inversion to pass on to the irreducible state.

The great fatality which occurred in the cases detailed by the early writers upon this subject, was doubtless owing to the obstetric practice being in the hands of ignorant females: the inverted organ was frequently mistaken for the head of a second child, or for some preternatural tumour which ought to be removed, and in consequence great mechanical violence was used, and the constitutional powers greatly exhausted before a surgeon was called upon. In the present day the result is, or ought to be, different; the tumour may be easily reduced, and the result is generally successful.

From the number of cases detailed, inferences, in a great measure correct, may be drawn of the nature of the causes which have produced it, and also of the circumstances which attend it. But notwithstanding the great advantages arising from this source, we find practitioners resting satisfied by merely repassing the tumour through the os externum into the vagina; by which those miseries already alluded to are en-

tailed upon the unhappy patient, or else her life is the forfeiture, and the grave conceals his ignorance.*

Systematic writers in general adopt the opinion entertained by their predecessors of the circumstances which occur at the time of the inversion, more from respect to authority, than an accurate analysis of the cases which have been published. But in some material points of practice a great difference in opinion is to be found.

CASE I.—Mr. Wood, my late partner and respected relative, was requested to visit a poor woman who was delivered by a midwife about two hours previously. He found the patient very much exhausted, with pallid countenance, and a cold surface. Upon a vaginal examination he found a large tumour, with the placenta attached, and which he at once recognized as the *uterus inverted*. There was no hemorrhage or convulsion; the funis was of the ordinary length, and was not entwined round any part of the child. The midwife assured him that she had made no effort to remove the placenta, nor could she attribute the occurrence to any treatment she had adopted. Mr. Wood detached the placenta, and then reduced the uterus, which he accomplished without any great difficulty. The patient went on well, and recovered without any further inconvenience, except that of a longer confinement.

CASE II.—The writer is indebted for the following case to his respected friend Mr. Mann, a truly intelligent practitioner.

“ The subject of this accident was Mrs. Birch of Great Bridgewater-street, a well formed, healthy young woman, and this was her third confinement. I was summoned to her on the 17th day of May, 1826, about three o'clock in the afternoon. I found her walking about the room, with the pains bearing down, and effective. In a short while after my arrival, whilst leaning forward on the bed, she was delivered of a fine healthy

* See Case 24. Dr. Ramsbotham's Practical Observations. Glasgow Journal, vol. i. p. 171.

male child ; from this position (as soon as the child was separated) she was removed carefully into the bed ; in less than ten minutes she had a slight pain or two. My patient expressed some fears lest the placenta ‘ *should stick,*’ but on my making an examination *per vaginam*, I distinctly felt the insertion of the funis into the placenta, and relieved my patient of her fears as to its being retained unduly. I had scarcely assured her all was likely to terminate well, when she was suddenly seized with a violent bearing down pain ; and on making a further investigation I discovered, what I took for the instant to be the placenta pushed forward by a second child’s head ; but having recourse to ocular investigation, I was soon undeceived in this respect, and found the uterus inverted, which had passed externally from the vagina, with the placenta attached to it. I felt very much alarmed for the fate of my patient. I first peeled the placenta from the fundus uteri, and then grasping the extruded part with my hand, I did not find it very difficult to re-introduce it into the vagina, and carry it through the os uteri. I followed with my hand, or rather pushed it forwards, when I observed it suddenly start from me as a piece of Indian rubber would. By a subsequent examination I found all the parts *in situ*. I was now called by the nurse to examine the state of my patient, which indeed was very alarming ; her face became suddenly pale, and bedewed with cold sweat ; her pulse was rapid and unsteady ; there was great prostration of strength, and a threatening of convulsions and death. Brandy and laudanum were immediately administered in free doses ; hot flannels and friction were applied to the extremities, &c. ; but it was more than two hours before I could consider my patient safe. She afterwards did well, and has since borne children.

“ The above is the only case which has ever occurred in my practice, and I would remark, *firstly*, that this inversion was entirely spontaneous, as I had not even taken hold of the funis at the time it happened. I mention this fact, because it was formerly considered that inversion was occasioned by pulling at

the funis ‘as you would at a bell-rope.’ *Secondly*, as there was no hemorrhage, and as the reinversion was effected in a few seconds, it is somewhat difficult, in my mind, to account for the sudden depression of the vital powers, amounting nearly to dissolution.”

CASE III.—Mrs. Capper, midwife, requested me to visit Mary Wilson, Mount-street, Knot Mill, who had been delivered forty-eight hours. I was informed her labour was propitious; that the placenta was naturally detached and expelled. There was no hemorrhage afterwards, nor indeed any other symptom which required further assistance. She was induced to send for me at this time, because the patient had discovered something protruding from the os externum, and which she, upon a first examination, considered to be a coagulum contained within the membranes. She applied a slight force, but it gave the patient great pain, and not succeeding in bringing it away, abstained from further violence. When I made a vaginal examination, I found a tumour of considerable size passing partly through the os externum; it was hard and resistant; externally it felt flaky; it was broader below than above. It was with great difficulty I could reach the os uteri, which tightly embraced the upper part of the tumour. The lochial discharge was greater than usual, and more sanguineous.

Although the case was somewhat obscured by nothing having occurred immediately after the labour, yet I was convinced, from a very careful investigation, that it was one of partial inversion; I therefore determined to make every effort to replace it. My exertions were continued for two hours, and only given up on account of the pain produced, and the exhausted state of the poor woman. Further attempts I considered would be dangerous. From this time her health declined. Her first symptoms were those of peritoneal inflammation, retention of urine, &c. The antiphlogistic plan was adopted, as general and local bleeding, fomentations, turpentine lotion, febrifuge medicines, &c. When the inflam-

matory excitement was subdued, I again attempted to reduce the tumour, but was again unsuccessful. Her health declined. She now suffered from sanguineous, mucous, and purulent discharge from the vagina, which produced great debility, diarrhœa, aphthous affections of the mouth, &c. These symptoms continued to harass her for six months, during which period I had in view the operation of extirpation as soon as circumstances would warrant its adoption. I frequently examined *per vaginam*, and found the uterus gradually lessen, until it acquired the size of a large pear. During the whole progress of the case the os uteri very tightly girt the neck of the tumour. The discharge was now more decidedly purulent. She now left town for about a month, and upon her return I again visited her. Her general health was improved; the vaginal discharge lessened, but still purulent. I made a vaginal examination, but could detect no tumour. I felt the remains of the os uteri, but no regular aperture; the upper part of the vagina formed a complete cul de sac. I inquired if she had discovered any substance passing from the vagina; she said she had felt something pass away, but thought it was "a lump of blood." Her general health gradually improved, and she lived several years. She died of cholera; and I regret much I was not apprized of her death until some months afterwards.

The case is truly interesting; it shows what resources Nature has in her own power. It justifies the operation of extirpation when, as in the present case, that period, so precious, has been passed by, when reduction can be effected. It may be thought that a spontaneous reinversion took place, as happened in the cases related by M. Delabarre and the justly celebrated Baudelocque.* The writer does not think it possible for this spontaneous change to occur, notwithstanding the ingenious explanation of M. Dailliez.†

* Gardien *Traité Complet D'Accouchemens*, &c. Tome iii. p. 318, §19.

† *Dissertation sur le Renversement*.

The os uteri acted here as a ligature, and induced the process of ulcerative absorption, by which the part was separated. The writer made every justifiable attempt to reduce the part, but has since thought that something further should have been tried, as he has been lately consulted in a case which was reduced on the seventh day after labour.* But a material difference existed in the two cases; in the one the os uteri was atonic; in the subject of the present history, it was rigidly constricting the cervix; and in such cases an incision made on each side of the os uteri would, in the writer's opinion, add to the chance of reduction. Millot has advised this incision in cases more advanced.† Nauche also advocates the same procedure.‡ Would the tobacco enema aid us in our attempt at reduction?

CASE IV.—I was requested by the late Mr. Dick to visit a patient who was in labour of her first child. The pains commenced at bed time the night before. The liq. amnii was discharged twelve hours before my visit, and I learned that the os uteri was then dilated to nearly the diameter of a crown piece. After the discharge of the water the pains became more feeble, especially when the patient was recumbent, but when she changed her position to walk or sit, they increased in power; on this account the patient was placed upon the lap of a female friend, and was delivered in this posture. After the child had freely cried, and the placento-foetal pulsation had ceased, the funis was divided; it was of ordinary length, and did not encircle the neck. The patient was now put in bed, and I placed my hand upon the abdomen, and found the uterus hard, and rather larger than is usual. I now took the funis in the left hand, and gently stretched it, and passed the finger of the right into the vagina to examine for the placenta, but was not able to feel it. Whilst I made the inquiry, Mr. Dick placed his hand

* Vide Case VI.

† Supplement à tous les Traites, &c. Tome ii. p. 262.

‡ Des Maladies propres aux Femmes, premiere Partie, p. 141. Boivin et Duges
Traité Pratique des Maladies de l'Uterus.

rather suddenly upon the belly, a strong forcing pain came on, and the woman exclaimed, "Oh! the after-birth is coming!" The placenta was now found to be rapidly advancing, and in a moment it passed through the os externum. I then discovered it was not the placenta alone, but this mass adherent to the uterus, constituting an extreme degree of partial inversion. The protrusion was sudden and forcible, and was attended by a bearing down effort. The tumour was about the size of a child's head, hard and firm, but smooth, being covered by the membranes. The placenta adhered to the fundus; little discharge took place, but the patient complained of being faint. I now detached the placenta, and easily accomplished it. I then compressed the uterus between the hands, and felt it to diminish in bulk. It was now passed through the os externum, and easily carried up, until the vagina was made tense. Resistance was now found to its further progress, but keeping a steady bearing upon it for some time, it gradually gave way, and persevering, it was passed through the os uteri. The hand was carried into the uterine cavity, and retained until contraction took place. The os uteri grasped the wrist, assuring me that regular action was in operation. The patient recovered without any interruption. She was desired to keep the recumbent position longer than is usual, and to pay great attention to the bowels.

In an attempt to reduce an extreme degree of partial inversion, it will be found that the tumour will freely pass through the os externum, and as only one hand can be admitted into the vagina, the chief compression should be effected whilst it lies externally. And as the upper part of the vagina descends along with the uterus, no real effect can be produced until it is made tense by carrying this organ upwards. When it arrives at this point, resistance is met with, but by keeping a steady pressure upwards, the inflected portion of the cervix then yields, and it gradually recedes, followed by the hand of the operator, until the reduction is completed. If, instead of the plan now

recommended, a forcible and quick attempt be made, the vagina may be separated from the uterus, and a fatal injury inflicted.

CASE V.—Mrs. Jeal, midwife, desired me to visit Mrs. ———, Union-street, Ancoats, who was in labour of her fourth child. The labour was tedious, and when I made a vaginal examination, I discovered the cause of tediousness to be a contracted pelvis. I found the os uteri dilated, and the head of the child lying at the brim of the pelvis. I applied the long forceps, but not succeeding in bringing the head down, I determined upon using the perforator and crotchet. The child delivered, I divided the funis: it was of the common length, and was not entwined round any part of the child. In about half an hour the poor woman said she had a “violent pain, and the after-birth was coming.” As I was engaged with the instruments I desired Mrs. Jeal to ascertain whether this was the case. She was then absent from the bed, but at my request she went to make the examination, and immediately exclaimed, “there is another child, for the head is advancing.” I made the necessary examination, and found a large globular tumour, hard and resistant, with the placenta attached nearly in the centre. This I at once discovered to be an inverted uterus. The fundus had partially passed through the os uteri. I introduced the hand into the vagina, and pushed the tumour upwards without much difficulty. The depending part of the tumour seemed to retire from my hand with considerable force. I then detached the placenta, which was not withdrawn until I was assured that the uterus would contract. There was no flooding, faintness, or convulsion, and the patient recovered without an untoward symptom.

CASE VI.—(Communicated by a friend.) The subject of the present case was about 40 to 43 years of age. She has had a number of children. Her complexion is sallow, and habit leucophlegmatic. During the whole period of this, her last pregnancy, she suffered very much from irritation in the pelvic organs. Her bowels were almost always constipated.

Complained of great weakness during the whole time she carried the child. When called to her in labour, I was told she had suffered grinding pains for several hours. As the pains increased, I proposed to make a vaginal examination. I found the os uteri dilated, and the head of the child low down in the pelvis. The labour advanced, and in about four hours after my arrival, the child, which was living, was expelled. The funis was divided as soon as the respiratory function was established. It was of the ordinary length, and was not twisted round any part of the child's body. A discharge of blood now occurred, but it was considerably less than what happened in two of her former labours. Considerable uterine contraction now came on, and the placenta was therefore immediately removed, and the discharge greatly abated. When I made my evening visit I found she had great difficulty in voiding her urine; I therefore passed the catheter; and at the same time satisfied myself that the uterine tumour was to be felt above the pubes. She was very low, and grew weaker for two or three days; and as the lochial discharge was greater than usual, I was induced to make an examination, and found a tumour low down in the vagina, and indeed protruding through the os externum. From the imperfect information I gained, I at first thought it was procidentia uteri. I attempted to return it, but did not succeed. I now proposed a consultation, but it was not acceded to until several days elapsed, when the pain was very great from the trials made to reduce it. An eminent surgeon was now consulted, and agreeing with me that the case was most serious, proposed having a third party brought into consultation. Agreeing that it was a case of inversion, we determined to attempt the reduction, and in about fifteen minutes it was happily accomplished. She has gone on very favourably ever since, presenting no threatening symptoms; only complaining of extreme weakness, and a slightly irritable bladder.

In the case above detailed there was no effort made to remove the placenta by pulling the funis.

Cases are only valuable inasmuch as they illustrate practical points. Those above detailed prove that inversion is easily remediable by perseverance at the time of its occurrence, and also after the lapse of several days. This happy result, in connexion with others, I shall have to allude to, is much more encouraging to the practitioner than the opinion of Dr. Denman would lead him to anticipate.

This accident has been attributed to causes purely mechanical, the uterus being unresisting, and passively obedient to their influence. The practice of pulling too early and violently at the funis, after the expulsion of the child, before the uterus has contracted, so as to detach and expel the placenta, has been generally considered as the cause of inversion. But we know that the accident happens before any force has been applied to the funis.* In Case IV. the descent was so rapid and forcible through the pelvis and os externum, that it would have been quite impossible to resist the unnatural action by which the organ was carried down.† It has occurred, when the patient had been delivered of a dead child, the funis so putrid as to break with a very slight effort.‡ It has been found before the cord was separated, and the child given to the nurse.§ In the practice of Ruysch this circumstance took place after he had extracted a dead child, &c.|| These circumstances show that there is a power inherent in the uterus to become inverted. The pulling of the funis is so common a practice amongst our midwives, and done without the least consideration of the condition of the uterus, that if it was so frequent a cause as is

* Vide cases already cited. Also Dr. Albers Duncan's *Annals of Medicine*, vol. v. p. 390. Mr. Dickenson's case, *Med. Gazette*, No. 372. Mr. Windsor, *Med. Chir. Trans.* vol. x. p. 359. Dr. Dewees' *Cases*, *Essays on various Subjects connected with Midwifery*.

† Vide Smith, *Med. and Phys. Journal*, vol. vi. p. 503.

‡ Brown, *Memoirs of London Medical Society*, vol. v. p. 292.

§ Welsh, *Med. and Phys. Journal*, vol. v. p. 451.

|| Observation, *Anatom. Chirurg. Obs.* x. p. 13. Translation, p. 34.

usually stated, inversion, instead of being one of the most rare, would be the most common accident in midwifery. Some writers have thought that a short funis is a frequent cause of inversion; whilst others think, in order to act, it must be inserted in the centre of the placenta, and that this mass must be attached to the fundus uteri.* Now it is evident, if brevity of the cord is capable of producing so serious an accident, these peculiarities will greatly add to its influence. But amongst the published cases of inversion there is, so far as the writer knows, but one where this shortness existed.† It often occurs without diminished length in the cord; whilst, on the contrary, children are frequently born where it is very short, and yet no such event happens.‡ The funis has been ruptured,§ and the placenta disrupted,|| and yet the uterus was not inverted.

In order that the causes which have been now alluded to, could operate effectually to produce inversion, there must be such condition of the uterus present, that it becomes tacitly obedient to their influence. Most systematic writers, as also others, have supposed such to be the case. They have said that the uterus, previous to inversion, is in a state of extreme relaxation, exhaustion, or collapse, and that it offers no resistance to any force applied by the funis.¶ These opinions are

* Gardien.

† Dr. King's Case. Several coils of the funis were round the neck of the child, and it was also twisted round one arm. *Glasgow Journal*, vol. i. p. 17.

‡ The inquiries and deductions of the writer's respected friend, Dr. Churchill, are strong evidence of the truth of the above statement; and he is glad to have his opinions corroborated by so respectable a practitioner.

§ *Med. and Phys. Journal*, vol. liv. p. 205. *Giffard's Cases*, Nos. 92, 127, 175, 194, 199. *Perfect's Cases*, Nos. 109, 132.

|| *Ramsbotham, Practical Observations*, Part. I. Cases, Nos. 28, 31, 32, 33, 34.

¶ Mr. Ingleby, *Facts and Cases in Obstetric Med.* p. 221. Dr. James, American edition of Burns, Note 104. Dr. C. M. Clark, *Observations*, p. 15. *London Practice of Midwifery*, p. 310: Dr. Jewell, Dr. Denman, Dr. Francis, Dr. Gooch, Dr. Aitken, Dr. Spence, &c. &c.

at variance with that of the writer,* which is founded upon an analysis of the cases published and those detailed in this paper. The fundus came down with strong bearing pain, in some cases extremely violent. In Cases Nos. II., IV., V., detailed in this paper, the protrusion was forcible, and attended with a strong bearing down pain. In Dr. Cleghorn's case,† “the pains, so far from being alleviated by delivery, grew every moment more intense; those of the back, in particular, were so excruciating, and the throes, which the women call bearing pains, so violent, that the patient, worn out with fatigue and suffering, became alternately insensible and delirious.” And in Mr. Brown's case the pains are described as becoming “more excessive, with a degree of bearing down hardly to be conceived, and an actual inversion of the uterus took place.” Violent pain, bearing down, and forcible protrusion, are not characteristic of relaxation or collapse of the uterus, but are here, as before the expulsion of the child, the consequence of contraction. Other circumstances show that the uterus is not in a state of relaxation. Such is its distention previous to labour, that great contraction must take place to enable the inverted fundus to pass down through the pelvis and os externum. The fundus and body must at least be so contracted as to represent a body not larger than the size of a child's head, as we know that there is such an exact relation between the pelvis and the foetal head. The tumour has frequently been mistaken for the head of a second child,‡ and in some instances severely used before the error has been discovered;§ in others it has been compared in size to it.||

* Dr. Dewees' Essays.

† Medical Commentaries, vol. ii. p. 226. Brown, Memoirs of London Medical Society, vol. v. p. 202. White's, C., Treatise, p. 431. Smith, Med. and Phys. Journal, vol. vi. p. 502. White, Med. Comment. vol. xx. p. 218. Hamilton, Med. Comment. vol. xvi. p. 316.

‡ Cases II. and V.

§ Bartholin. Note in Burn's System of Midwifery. Ruysch. Dr. Hamilton, Med. Commentaries, vol. 16. Cowley, London Med. Journal, vol. vi. p. 366.

|| Mr. Dickenson's Case, Medical Gazette, No. 372. p. 351. Doctor King, Glasgow Journal, vol. i. p. 172.

A tumour which can be mistaken for a child's head, must have similar tangible properties, as firmness, hardness, &c. It has been described as the size of a fist,* of a large pear;† it has been compared to a printer's ball;‡ it has been found between the thighs of the patient the size of a foot-ball;§ compared in size and shape of the indentation at the bottom of a blacking bottle;|| and in one case after death it was found resembling “a cup with a number of plicæ or folds round it, and was so strongly contracted as to require the utmost efforts of myself and son, &c. to reduce it, &c.”¶ Mr. Ingleby says, “The vagina was filled by a very bulky round tumour, &c., and resembling a very large sized polypus.** Dr. Ramsbotham says, “the inverted uterus, which had now become flaccid, during life it had been firm and resistant.”††

It appears to the writer that the uterine pain, diminution of bulk, firm, resisting feel, sudden formation, and rapid protrusion, warrant him in the deduction, that the *fundus* and *body* of the uterus, so far from being in a state of *collapse* or *relaxation*, are really in a state of *unnatural excitement and action*. But this is not the case with the *os uteri*; on the contrary, it is soft and yielding, as we find that it offers no resistance to the coming down of the tumour, whose protrusion is forcible and rapid. If these statements be true, it is evident that the *fundus* and *os uteri* are in directly opposite conditions; the former is in a state of violent contraction, the latter in a state of relaxation; and that this relative difference in these two parts of the organ is indispensably necessary to exist where inversion occurs.

* Lamotte, Treatise, p. 497. Obs. 384.

† Dr. Hunter, Annals of Medicine, vol. iv. p. 367.

‡ White, Med. Comment. vol. xx. p. 247.

§ Smellie's Cases; Collect. 44, Case 4, p. 445.

|| Dr. Dewees' Essays, &c. Case 1.

¶ Dr. Merriman's Synopsis, Appendix, Case 3.

** Facts and Cases, p. 227.

†† Practical Observations, Case 24.

Dr. Douglass has endeavoured to prove that the structure and economy of the fundus and body of the uterus are different from that of the cervix.*

In natural labour the escape of the liquor amnii is succeeded by a permanent and general contraction of the uterus upon the child, increased as the several parts of its body are expelled. Now the fundus and body act powerfully, whilst the os uteri is completely overcome and obliterated. But when the child is expelled, this opening is diminished by the lips becoming again apparent, and which are to be felt soft and projecting. This change is also shewn in twin labours, for however soon the second child succeeds to the first, the uterine orifice offers a slight resistance; and in the expulsion of the placenta, as it passes through this opening, slight uneasiness, pain, and bearing down, are the result. But sometimes the regular and uniform contraction does not take place, from causes obscure; and many inconveniences follow.† There are several varieties of irregular uterine contraction, and in all, some part of the organ must be in a state of weakened action, whilst another is in a highly excited condition. It is not essential to inquire here which of these two states lays the foundation of the mischief. The uterus is divided, in hour-glass contraction, into two compartments: in the upper the placenta is uniformly found. A contraction sometimes takes place in the body and cervix, leaving a chamber at the fundus, in which also the placenta is always found. There again occurs a contraction in the os uteri, with deficient action in the body and fundus. This condition leads to a retention of the placenta, and in some instances it takes place after the placenta has been removed, giving rise to internal flooding. There are states of the uterus where some portion or portions contract with greater force than the rest. Now it appears to the writer that inversion is another instance

* Medical Transactions, vol. vi. p. 379.

† Dr. Douglass, Med. Trans. vol. vi.

of irregular contraction, in which the fundus contracts powerfully, whilst the cervix and os uteri are in a state of atony. It is obvious that if the fundus and body continued their action after the expulsion of the child, before the cervix and os uteri have regained their proper powers, that an inversion must take place. We find this exemplified in other organs; the power of expulsion is increased in proportion to the relaxation of their sphincters. Certain states of the bladder and of the rectum are examples. In natural labour the principle is well illustrated: and in the management of many cases of protraction, our treatment is particularly directed to the object of relaxing the orifice, and thereby increasing the energy of the other part of the uterus.

If the writer is not mistaken, the case related by his respected friend, Mr. Ingleby of Birmingham, is a further evidence of the view he has taken as to the excited state of some particular parts of the uterus when it becomes inverted; and although his inference differs from that of this highly respectable practitioner, yet he will, without further apology, transcribe the case.

“I was compelled, in conjunction with another practitioner, to apply the forceps under the disadvantage of uterine inertia; after the delivery of the child, there was no tendency to expel the placenta, but a portion of the mass having separated, a slight effort was made by the funis. The placenta descended considerably beyond the os internum, together with a quantity of the uterus, apparently the whole of its right side, the left not being sensibly depressed. Flooding ensued. At the moment we were rather perplexed; but the nature of the displacement became evident, and the inverted part was immediately returned, together with the placenta. The adherent portion of the mass was then separated without delay, and the case treated in the usual manner.”* Again, the way in which the fundus

* Facts and Cases in Obstetric Medicine, p. 222.

retired from the hand in Cases II. and V. and also in those related by Dr. Merriman,* are further illustration.

In labour, if it is to proceed regularly, a just balance must exist between the several parts of the uterus; and anything which irritates the fundus or os uteri, during the process, to produce violent action or rapid dilatation, are causes of irregular contraction.

In the cases of inversion which have been detailed, we find numerous sources of irritation mentioned, which are equal to the production of irregular contraction. Hastening the labour;† artificial rupture of the membranes;‡ patient delivered in a sitting position;§ rapid labour;|| mental alarm and agitation;¶ hasty extraction of the placenta;*** erect position during delivery;†† leaning forwards over the bed during labour;‡‡ premature giving way of the membranes;§§ tedious labour;||| pulling at the funis;¶¶ to these statements many more might be adduced, but they are sufficient to shew that great mismanagement has existed. They are causes which are capable of hastening or interrupting the parturient process. Many of these circumstances are introduced when put in force to increase the power of the pains during labour; they have a direct tendency to effect a too rapid, premature, and forcible dilatation

* Synopsis' Appendix, No. 30, Cases 1, 2, 3.

† Lofter, Medical and Physical Journal, vol. ii. p. 207.

‡ Welsh, Do. Do. vol. v. p. 450.

§ Charles White's Treatise, &c. p. 429, also Case IV. of this paper.

|| Dr. Albers' Annals of Medicine, vol. v. p. 391, Dr. Ramsbotham's Case, Dr. Dewees' Cases, 1, 3.

¶ Smith, Medical and Physical Journal, vol. vi. p. 503.

*** Dr. Hamilton, Medical Commun., vol. xvi. p. 616. Lamotte.

†† Dr. Cleghorn, Med. Commun. vol. ii.

‡‡ Case II.

§§ Dr. Dewees' 2nd. Dr. King.

||| Cases IV. and V. of this paper, Dr. Selcombe: Dr. Perfect's Cases, 71 and 72.

¶¶ Most systematic writers.

of the os uteri, and not a less tendency to produce spasmodic and irregular contraction in the womb.

The rapid extraction of the child's body after the head is expelled, is a practice fraught with evil, it is one which the writer has witnessed frequently, notwithstanding the strong language used upon this subject, and the able exposition of our late highly gifted townsman, Mr. Charles White. The temporary cessation of the expulsive efforts after the extrication of the head, is of the utmost importance to the female; and the abrupt removal of the stimulus of distention given to the os uteri, by the shoulders and body of the child, leaves the organ without excitement before it has recovered its power of contraction. Fainting and hemorrhage frequently occur in consequence of the hasty extraction of the child's body, and result from the sudden removal of uterine and abdominal distention.

From what has been stated, it may be concluded, that quick labour, whether natural or artificial, a disturbance of this process in any of its stages, or any of those circumstances which produce irregular contraction of the uterus, are singly, or combined, the causes of inversion.

(*To be continued.*)

ART. III.—*Observations on Dr. ABRAHAM COLLES's Work "on the Venereal Disease, and on the Use of Mercury."*

By RICHARD CARMICHAEL, M.R.I.A., Honorary Member of the Royal Academy of Medicine of France, &c. &c., and Consulting Surgeon of the Richmond Surgical Hospital, &c. &c.

IF the "Practical Observations on the Venereal Disease, and on the use of Mercury," lately published, had come from the pen of a person less influential than that of my friend, Doctor A. Colles, I should have allowed them to pass without any remarks of mine; but his doctrines are so much in opposition to

those which I espouse, that although he has not honoured me so far as to notice any of my published opinions on the subject, yet I feel that his observations have been in many places so *pointedly* opposed to them, that I should be deficient, not only in that just portion of *amour propre*, which every man ought to possess, but in my duty towards the public, were I to allow his to pass unheeded, and to permit others to suppose that I had changed my mind, or acquiesced in doctrines which have not even the allurements of novelty to recommend them.

I trust that the observations I am about to make will, be found devoid of all the acrimony which a desire to achieve a triumph is so likely to induce, but that they will be made in that cool spirit of philosophical investigation, where the object alone is to ascertain a truth of considerable practical importance; and I beg particularly of the author, not to consider them in the light of an attack upon his opinions, but in that of a defence of my own.

The first passage, in the import of which I cannot agree, occurs in the first chapter, which treats on the "Natural History of the Venereal Disease." In it we find John Hunter eulogized in the terms he so amply deserves, as having ascertained many important facts relative to the natural history of the venereal disease not previously known. But then, as the author observes, he left much for his successors to achieve, and exclaims, "does it not appear strange, that subsequent writers have not made some efforts to supply those deficiencies." He then proceeds to account for the little progress that has been made in this inquiry, as arising from the universal exhibition of mercury, which interferes with the natural progress of venereal complaints; for "every practitioner is so impressed," he observes, "with the sense of duty to do all in his power for the relief of his patient, that he cannot, consistently with that feeling, withhold such medicine, or refrain from such means as may be best adapted to the case."* Now

* Colles, p. 4.

this conscientious feeling has, no doubt, prevented our author from taking any steps towards ascertaining the natural progress of venereal complaints, when not interfered with ; but as a feeling, perhaps as strong, has prevented others from having recourse to mercury in many forms of the venereal disease, so we must look to the observations of the latter, or to those who abstain altogether from the use of mercury, for information on the point in question. And I would here beg leave to ask, has no useful additional information in the natural history of those diseases been obtained since Hunter's time, by those practitioners who refrain from the use of mercury ? Is not the classification alone of the various forms of venereal disease, grounded on the nature of the eruption, a great and important step towards a just knowledge of their natural history ? In Hunter's work, we find these eruptions all jumbled together under the general term of copper-coloured blotches, which is as little significant of their character as the term *dartre*, so generally applied by our neighbours to every variety of cutaneous disease. In Hunter on the venereal, we find no mention of papulæ, pustules, tubercles, rupia, psoriasis, or lepra ; all those forms of eruption, indicative of different forms of the disease, and often requiring different modes of treatment, are all classed under the one general and undistinguishable term of copper-coloured scaly blotches. Now Hunter is not to be accused of want of discrimination for this omission ; because, at the time in which he lived, there was but little precise knowledge, and no accuracy of nomenclature, respecting cutaneous eruptions. But when the profession became enlightened on this subject, chiefly by the labours of Dr. Willan, the information thus obtained was applied with manifest utility towards a just classification of the different forms of venereal disease.

That a classification, grounded on the character of the eruption, is not only in accordance with nature, but the most practically useful one which could be devised, is apparent from the following circumstances :—

1st. When a practitioner meets with a patient affected with a papular eruption, either in its early stage, when it presents itself in the form of pimples with acuminate heads, containing matter; or in its advanced stage, when the spots have desquamated, and present one of the appearances which Hunter calls copper-coloured blotches, he may be certain that he has only a disease of easy management to contend with, and that under suitable treatment, he may assure his patient with confidence of a certain and speedy recovery.

2ndly. If we meet with a pustular eruption which terminates in *superficial ulcers and not in desquamation*, we may be certain that we have a much more formidable disease to manage.

3rdly. If there is offered to our consideration a case of pustular eruption, mixed with tubercles, which terminate in *deep ulcers which spread into a phagedænic margin, and form those crusts termed rupia*, we may be certain that we have the most unmanageable and destructive form of venereal disease to contend with. For under the most judicious mode of treatment, we too often fail in curing the malady, which annually destroys numbers, after enduring a lingering and loathsome catalogue of disgusting ailments. For it is in this form of the disease that we meet with extensive ulceration engaging the velum, uvula, tonsils, and back of the pharynx where it lies upon the bodies of the vertebræ, ulceration of the nares, destruction of the bones of the nose, and also ulceration of the larynx, the usual precursor of laryngeal phthisis.

4thly. The scaly eruption comprising psoriasis and lepra, is a form of the disease which is in general manageable; for unlike the pustular and phagedænic forms of venereal, it yields with certainty and quickness to the influence of mercury. It may, however, be accompanied by deep ulceration of the tonsils, and very obstinate nodes; but whether ulceration of the larynx and of the nares attends it, is more than I can say, never having wit-

nessed it. This form of the disease, which, from the term “copper coloured scaly blotches,” was probably most frequently met with in Hunter’s time, is now comparatively seldom to be seen. It is often confounded with other forms of venereal eruption, because papulæ and pustules, in their desquamating declining stage, when the virulence of the disease is considerably exhausted, assume a scaly, copper-coloured appearance ; and even rupia evinces the same tendency, when the disease is on the wane. But I have never seen an instance (although Mr. Colles adverts to several) of “a papular or scaly eruption degenerating into rupia.”

The tendency of all venereal eruptions is, as the disease is becoming exhausted, and is gradually yielding to the powers of the constitution, to become scaly. And when in this state, no matter in what form the eruption commenced, I never saw mercury do harm, but always observed the most decided and quick amendment to follow its adoption,—a general fact which must be a great comfort to those who are either too indolent, or too sceptical, to think it of any consequence to pay attention to the precise character of the eruption.

From the facts above stated, I have a right to deny the justice of the sarcasm of our author, that all who treated of the venereal disease since Hunter’s time, have added no additional information relative to its natural history. It is a most important practical point, and, coupled with the natural history of venereal complaints, to be able by an accurate diagnosis, grounded on the character of the eruption, to prognosticate the probable event ; to be able to state whether the patient will recover in a short time, or whether he may linger for years, and finally sink under the disease ; and also from the nature of the eruption to be able to decide on the most appropriate mode of treatment.

I have not in these observations on the natural history of venereal diseases, insisted upon the much disputed point of

a particular form of primary ulcer being followed by a particular or corresponding form of eruption. And I have refrained from doing so, because, though my opinions remain unshaken on this head, and are supported by a host of observations, yet as it is still *sub judice*, I am unwilling to assume it as a fact established in the natural history of venereal diseases.

But notwithstanding this concession, and that Mr. Colles declares, “that after a long and careful observation, he had not been able to trace particular forms of eruption to particular forms of primary ulcers,” I must also declare, that nothing is more decidedly connected, in my mind, than the links which exist between gonorrhœa virulenta, a patchy excoriation of the glands and prepuce, and the simple fungous-looking ulcer without induration, with the secondary symptoms—papular eruption and all its concomitants, as laid down in my work on venereal diseases; and, again, the connexion which exists between the primary phagedænic or sloughing ulcer, with venereal rupia and all its dreaded companions, is equally evident.

That the papular and phagedænic forms of venereal disease, as described in my work, combine in each a concatenation of symptoms both primary and secondary, totally dissimilar in appearance, in character, in duration, and in susceptibility of being acted upon by medicine, is obvious to any man even of a limited experience, and must be acknowledged as such by every candid mind solicitous after truth.

Mr. Colles's reasons for dissenting from this doctrine, I shall give in his own words:—“Firstly,” he says, “I have not unfrequently observed varieties of eruption exist together in the same individual; for example, I have seen small venereal lichen on the face, while a large form of papular eruption occupied the trunk and the extremities; sometimes, also, I have found spots of a pustular character scattered through a general crop of the papular eruptions. Secondly, I have noticed, as a very frequent occurrence, that when the first eruption has been removed, either by the use of mercury, or by other means, that the second

crop has proved of a different kind ; thus when the first eruption was of that small pimply kind which resembles measles, it has been succeeded by a papular eruption, and this again by a pustular crop. And, thirdly, by injudicious treatment ; for example, by the excessive use of mercury in bad habits, any one other form of eruption may be made to degenerate into one which is most obstinate and severe, namely that of rupia."

To these observations I reply, that small papulæ on the face and large ones on the trunk, in the same individual, do not evince any difference in the nature of the eruption on these two parts, but only a difference in its intensity. The same objection might be urged against the identity of the itch, small-pox, or any other eruption, because it happens to be more severe in any one part of the body than on another. Some explanation is perhaps required with respect to the nomenclature of venereal eruptions. Papulæ have constantly acuminate heads containing matter, which some might call pustules ; but the true diagnostic distinction between venereal papulæ and pustules is, that the former end in desquamation, the latter in superficial ulcers.

If a case presented itself in which pustules that terminated in ulcers were intermixed with papulæ, I would say the patient was afflicted with the pustular eruption ; always designating it by the term which signified the most severe and dangerous spots which appear on the patient. For in the pustular eruption we often meet with papulæ, and in the phagedænic eruption we often see the rupia, which marks the disease, intermixed with both papulæ and pustules ; but the first, in my judgment, indicates the true nature of the eruption.

In the same manner, in small-pox, we often observe papulæ intermixed with the pustules, which latter constitute the characteristic signs of the disease, for no person thinks of calling small-pox a papular, but a pustular, eruption.

With respect to the second objection, I may briefly observe, that the *tendency* of all venereal eruptions in re-appearing,

either a second, third, or fourth time, is to assume the scaly form. But although I have been many years attending to the natural history and the progress of this class of diseases, I cannot call to my recollection a single instance, even under "injudicious treatment, and the excessive use of mercury," of a papular eruption being succeeded by a pustular crop, or this last by rupia.

The successive crops of both the papular and pustular eruptions have, as I have just said, an obvious tendency to assume the scaly character, in which they appear like copper-coloured blotches, and even rupia itself has the same tendency; but with this modification, that the spots exhibit the appearance of raised rounded tubercles, of a scaly dark red or copper colour, of which we possess many excellent delineations amongst the admirable collection of drawings belonging to the Richmond Surgical Hospital.

At page 76, I find the following observation—"We shall now consider the proper treatment for a case of true venereal ulcer, the Hunterian chancre:—The local treatment should, in my opinion, be confined to the most bland and mild applications, such as cannot in any manner alter the features of the ulcer, for I am certain that many useful indications which may serve to guide us in the administration of mercury, are to be derived from observing the changes which these ulcers undergo through the agency of that medicine; these changes, and their corresponding indications, I shall hereafter advert to. I am further confirmed in this opinion from having observed that little or no benefit is derived from a contrary practice; thus, I have known a chancre completely cut out on the first or second day after its appearance, yet the occurrence of secondary symptoms was not prevented."

I cannot agree with my esteemed cotemporary in this reasoning or advice; for although *he* may have seen a chancre extirpated on the first or second day after its appearance, and yet secondary symptoms not prevented, yet I have seen, on the con-

trary, so many hundred instances of primary venereal ulcers treated by the free application of lunar caustic, with the view of destroying a surface secreting a morbid poison, which were not followed by constitutional symptoms, that I cannot but look upon this general exemption, by the mode of treatment mentioned, as a proof that I acted on a right principle. But in order to cut off infection, the earlier it is attempted the better. We should also recollect, that during the first two or three days, and whilst the ulcer is still excavated, and secreting a thin, ichorous discharge, it is in its most virulent state, and, therefore, most likely to infect the constitution. For we know, from the practice of inoculating vaccine and variolous matter, that the earlier the poison is taken, and before the matter becomes purulent, the more certain we are of imparting the poison;* and, therefore, it follows that the sooner we prevent an ulcer from secreting a highly infectious matter, the more likely are we to prevent the contamination of the system; and this is of more consequence than withholding our hand with the view of receiving "useful indications, which may serve to guide us in the administration of mercury."

Concerning the prevalent exhibition of mercury, Doctor Colles observes: "In my opinion, nothing can more clearly establish the claims of mercury to be considered as a *specific* for this disease, than the frequent instances of cures made by it in the hands of those who must employ it at random; and very frequently on patients as careless and incautious as the prescriber is ignorant and pretending. It is wonderful that an engine so very powerful could have been so long wielded by the hands of the inexperienced, the injudicious, the uneducated and unprincipled, and yet should not have committed more havoc than it has done." But surely Dr. Colles does not mean to as-

* The satisfactory experiments of inoculation of venereal matter, by Mr. Evans, detailed at p. 80, 2nd edition of my work on the Venereal, establishes this view of the subject.

sert that mercury has not committed most dreadful havoc in the hands he alludes to. There is scarcely a day that we do not witness instances either in public, or in private practice, of the injurious consequences arising from the abuse of this most powerful medicine; but I will not limit this imputation to the “injurious, the uneducated, and the unprincipled,” for I assert, that as long as mercury is blindly administered even by the judicious, the educated, and the principled, under the deceitful and empirical notion that it is a “*specific*” for the cure of all venereal complaints, and not given as a medicine of unequivocal and ascertained powers, on sound pathological principles, we are not likely to improve medicine as a science, enhance our own characters, or what is of still more consequence, benefit our patients. Are we always to be swamped by the obsolete axiom, that mercury is the only and appropriate cure for every form of venereal disease? Mr. Pearson formerly escaped from the many obstacles he met with in practice that militated against this notion, by christening those refractory symptoms that did not yield to mercury, syphiloidal, or sequelæ of syphilis; Mr. Mathias by calling them mercurial; Mr. Abernethy, pseudo-syphilitic. And now Doctor Colles tells us, that when mercury fails it is because it has been “injudiciously employed;” either in too small or too large quantities; “for too short or too long a time;” or that the patient has not been “duly prepared for its use;” or that he has taken it in a negligent manner, “and not under a sufficiently strict regimen.” It seems *never* to enter into his mind that when it fails, it may be *because it is an inappropriate or injurious medicine* for the form of disease under which the patient labours; or that it has been administered at an unfavourable period, for instance, during the continuance of the eruptive fever.

Thus at page 83, we find the following observations with respect to the treatment of primary ulcers:—“Although I have repeatedly stated, that when ptyalism has been established, the further use of mercury becomes comparatively safe, yet it may

happen, either from the mercury being continued too long, or too largely, in a weakly system, or from a state of fever induced by other causes, that a chancre which has made some progress towards healing, will take an unfavourable turn, and assume a phagedænic or a sloughing disposition."

Now here the ulcer, it seems, unluckily began to assume a phagedænic or a sloughing disposition, "because the patient had taken mercury too long or too largely." But if our author had not been blinded by his prejudices, he would have said, because mercury is not an appropriate or fit remedy for any primary ulcer which shows a disposition either to the phagedænic, or the sloughing process; but this would be expecting too much, for it would lead to an admission that there is a difference in primary ulcers indicating the necessity of different modes of treatment. Continuing his observations on these obdurate ulcers which so provokingly resist the infallible powers of mercury, we read at the next page but one, (85,) that "about twenty years ago, when I had charge of a large number of soldiers labouring under the venereal disease, who were received into the attic wards of Stevens's Hospital, I attempted the treatment of such cases by throwing in mercury largely and suddenly; but whether it was owing to the want of a judicious plan of using mercury, or to the bad habits of the men, induced by intemperance and dissipation, I know not; but I freely admit that with many this practice was not successful. However, the success of the two plans, that by mercury and that by the antiphlogistic regimen, was so evenly balanced at the time the military hospital was broken up, that I was quite undecided which to prefer. About this time I learned the use of the black wash, which has rendered such essential services in the early periods of this condition, that I have not since repeated the experiment of administering mercury through the constitution. Many of the cases treated with mercury were cured without the slightest destruction of any part, but this was purchased by the certain and severe sufferings of a violent salivation. Some

escaped with the loss of part of the glands, and some few had the penis destroyed down to a level with the pubes. Possibly the mercurial treatment would have been the more frequently successful, had I more constantly used venesection and other evacuations as a process preparatory to the use of mercury. There is one condition of the sloughing penis which I look upon with total despair of being able to afford any means of arresting its progress until it has destroyed the entire penis down to the pubes: I mean that condition in which the sloughing part is so soft, as to resemble melted tallow when beginning to form into a solid. I have never seen the progress of this arrested, even for a moment, by any local or constitutional means hitherto employed." And yet, at the very period alluded to by Doctor Colles, my work on venereal diseases was in the hands of every surgeon, and he would there have found a number of cases in which this form of ulcer was successfully treated, and the absolute necessity *emphatically* insisted on of abstaining altogether from the use of mercury. But I feel grateful for the candour of Doctor Colles's admission; and although I widely differ from him in my opinions, yet I have the utmost reliance on the honesty of his statements. Thus we find, that even in his hands, mercury failed to benefit phagedænic ulcers; but surely he of all others should not attribute this failure either to a neglect of the preliminaries of "venesection and other evacuations;" or to an "injudicious plan or mode of using it;" or even to the "bad habits of the men." For it is well known that soldiers being well fed, cloathed, and exercised, possess far superior constitutions to the generality of the inmates of hospitals. His reliance upon his favourite medicine, it seems, therefore, would have been sadly shaken, had he not at this time learned the use of black wash, the well-known combination of calomel and lime water. This fortunate discovery saved his confidence in the powers of mercury; and I believe, at the same time, not a few unworthy *members* of society from being reduced to mere stumps, if not uprooted altogether from their pelvic origins.

The good effects which so frequently arise from a spontaneous hæmorrhage in stopping the progress of a phagedænic ulcer, is next adverted to. This fact I particularly stated in my work, (at p. 166, second edition,) and therefore recommended (at p. 182,) the removal of the jagged and uneven edges of such an ulcer by the knife, which induces a bleeding that succeeds often in checking its further progress. But as our author does not condescend to notice the works of any man since Hunter's time, I have no right to complain of his disregarding this, and some other points of practice, which I believe originated with myself.

In the next page, (87,) even after the ulcer is induced to heal, he recommends a course of mercury, with a view of protecting the constitution. Now I beg to protest against this practice, as I have frequently seen a phagedænic ulcer, after it had been healed, break out afresh, as soon as the mercury had affected the system, as if to evince how inimical is its use in this form of venereal disease.

In the same page, we find that a full course of mercury is recommended for the cure of chancreous excoriation,—a primary affection, which I have mentioned as one of the causes of the papular eruption.

This recommendation, I acknowledge, astonished me; as I thought that such practice had been long since relinquished, having myself been in the habit of treating it with some mild astringent wash, and gentle aperients, during the last twenty-five years, a practice which usually removed the complaint in three or four days, the most obstinate case seldom extending to a week; and I found that constitutional symptoms as seldom occurred after this treatment, as when I had been in the habit of using mercury. On looking over the cases which Dr. Colles gives as illustrative of his mode of managing those ulcers, we find that Mr. W. (p. 88) used mercury in large quantities for the cure of this simple complaint, and that he even took five grains of blue pill, or three grains of calomel, three times a day,

while he rubbed in mercurial ointment every night. The next case, (Mr. B.,) who was affected with so mild an excoriation, that he was treated with cold water at first, and afterwards was put on a mercurial course, which, however, did not prevent the accession of constitutional symptoms. He, however, it appears, got worse under the use of mercury, and it was laid aside in consequence of an excoriation occurring about the anus. The patient then got well, and had not any return of his venereal symptoms.

Now, those who have been dosed with a full course of mercury know full well that it is really no joke, and it appears to me to be rather too serious a measure to inflict for a complaint, which, under the mild means pointed out, may be effectually cured in a few days ; particularly as we find, by Dr. Colles's own shewing, that mercury, however largely given, is not always successful in preventing the accession of constitutional symptoms.

Before I take my leave of his observations on primary ulcers, I must make some strictures on the following passage, which was obviously intended as a hit at my peccadilloes.

“ I shall not attempt any farther description of the various primary venereal ulcers which are daily to be met with ; much less shall I undertake any classification of their endless varieties, or of the course with which each peculiar form has been supposed to run. They will be found to differ so constantly, that very rarely shall we find any two of them to correspond accurately with each other ; not merely at their commencement do they present such dissimilar characters, but in their different stages towards healing they will be also found to deviate most strangely from each other, more particularly so when they have been treated by mercury.”

Now, although I perfectly agree with our author that it would be impossible to describe the endless varieties of appearances which ulcers present, because every ulcer may be modified by age, constitution, mode of living, local and constitutional

treatment, (particularly by irritating applications and mercury,) yet I contend there are at least four classes of venereal primary ulcers exhibiting, *ab initio*, distinct and characteristic appearances: 1st, the mild fungous-looking ulcer, devoid of induration or phagedæna; 2nd, the ulcer with raised defined edges and smooth surface, without granulation; 3rd, the phagedænic and sloughing ulcer; and 4th, the ulcer with a hardened base, or the true syphilitic chancre of Hunter. Now these four classes of ulcers may be so altered by the causes I have mentioned as to present interminable varieties; yet when we remove the inflammation, and wait until the mercurial irritation (the most common causes of these varieties) subside, we will in almost all cases be able to recognize, in the ulcer under consideration, one or other of these four venereal classes.

We now come to the chapter which treats of venereal affections of the throat, and we find that our author attributes the great variety of ulcers in this part to the *manner* in which mercury has been employed. But it is better to let him speak for himself.

“In my judgment, the great varieties in the appearances and nature of venereal sore throats, are occasioned by the manner in which mercury has been used, either for the cure of primary, or for the treatment of the secondary symptoms. No doubt when mercury is used injudiciously, and in a manner unsuited to the general health and condition of a patient, we shall find that it causes more mischief, and produces more strange changes in the venereal ulcers of the throat, if the patient be of a highly scrofulous, or of a very delicate habit, than if he be of a vigorous and healthy constitution.”

Now this conclusion of our author is completely subverted by the facts which the anti-mercurial treatment has enabled us to observe; and therefore, as I before said, it is only amongst the anti-mercurialists that we can learn any thing certain about the natural history of the disease. For we meet with great varieties of venereal sore throats in those cases where not a grain

of mercury had been used, and varying in mildness or severity in correspondence with that of the eruption which precedes or accompanies them. These have already been noticed in this paper, and sufficiently enlarged upon in my work on venereal diseases.

But I acknowledge that the injudicious use of mercury in cases unfitted for exhibition multiplies, *ad infinitum*, not only the variety but the severity of the ulceration. And when our author recommends a course of mercury for every venereal sore throat (for he does not acknowledge distinctions) "of eight or ten weeks' duration," (p. 122,) we may readily conclude what the result must be in the papular or phagedænic form of the disease. In the first it is not only unnecessary and injurious, but in the latter such a sweeping recommendation is absolutely destructive.

The case of James Johnson, (p. 133,) admitted Jan. 10th, is an excellent sample of the phagedænic venereal disease: rupia, and dark-brown scabs on the forehead, eye-brows, and nose, and scattered over the entire body. "The arches of the palate, uvula, and tonsils deeply ulcerated; the entire surface of the pharynx converted into, or covered by a soft slough, like half-boiled flummery, great emaciation, and weakness." This man was repeatedly salivated, before he was admitted into Stevens's Hospital, in other institutions. He was treated with sarsaparilla, nitrous acid, and lotions of nitrate of silver. On the 26th, ten grains of mercurial ointment were ordered to be rubbed in; the other medicines to be continued.

Feb. 6th. The ten grains of ointment were used *only every second night*, when diarrhœa set in, and the usual remedies for it were directed. We are not told whether the ointment was continued; but we find the ulcers were nearly healed; and "though he swallowed well, the fluid regurgitated through his nostrils whenever he drinks; and that for ten days past has complained of pains, &c., profuse night sweats, sleeps badly, appetite declining." The author then observes, "it is unneces-

sary to prosecute the details of this case farther." I believe not. And this is an instance selected as an illustration of the successful exhibition of mercury ! Verily a man must have unbounded faith in his panacea, who could produce the treatment of this case as an inducement worthy of imitation. The amendment is attributed obviously to the ten grains of mercurial ointment rubbed in every night for ten nights, and afterwards only every second night, although, at the same time, sarsaparilla, nitrous acid, and strong local remedies were employed.

The case which follows, (p. 138,) of an apothecary, who had a similar ulcer of his throat, so deep and extensive as to cause exfoliation of the ring of the first vertebra, is as strong an illustration as I could wish of the ill effects of mercury in this form of the disease. But I shall not make farther observations upon these and many other cases equally to my purpose ; but merely mention my surprise, that if a judicious use of mercury is capable of curing the phlegmænic venereal disease, how it happens that we are seldom without some cases of this form of the malady in the Richmond Hospital, which had been previously many months in Stevens's Hospital, where, no doubt, mercury had been tried in the most " judicious manner." Now, I do not mention this by way of triumph, but as a fact, to show that mercury is not the appropriate remedy for this form of venereal disease ; nay, that it is positively injurious and destructive.

In reply to this, I perhaps may be told that many patients are admitted into Stevens's from the Richmond Hospital—unsuccessful instances of the non-mercurial treatment. If so, I shall not feel in the slightest degree hurt by such a rejoinder ; only premising this, that we of the Richmond are only so far non-mercurialists, that we do not give mercury in every form, or in every stage of venereal complaints, nor in any, except those few in which its use may be resorted to with advantage, as distinctly pointed out in my work.

In Chapter X. our author contends for the use of mercury in venereal hectic fever, and also in that fever which precedes

and accompanies venereal eruptions. His words are : "I believe it is a pretty prevalent opinion at the present day, that when a venereal eruption is coming out, we should withhold mercury until the eruption be completed, lest we interfere with or interrupt that process. Another reason for withholding this medicine in such cases is, that the fever which accompanies the eruption is considered as unfriendly to the anti-venereal action of mercury. Now I am convinced, by repeated observations, that this rule not only may be departed from, but that it cannot be followed with advantage to the patient."

This doctrine is supported by some cases, in all of which mercury was given during the eruptive fever in minute doses ; for instance, two or three grains of blue pill once or twice a day, or ten grains of ointment rubbed in every night, or only every second night, are the doses he prescribes. When he speaks of hectic fever, I presume he alludes to that fever which is an attendant upon those constitutional symptoms which harass and wear out a frame already nearly exhausted by useless efforts to overcome the disease. Now under such circumstances, if the eruption were scaly, and thus indicating that the disease was obviously on the decline, I should not feel the slightest objection to the cautious exhibition of mercury, having, however, first tried sarsaparilla in conjunction with the *hydriodate of potash* ; which latter medicine I consider the most *powerful auxiliary* for the cure of the constitutional symptoms of the phagedænic disease, that has come into notice since I last published on the venereal. But as to the propriety of exciting mercurial action, whilst an eruption is making its appearance, attended with an eruptive fever, I decidedly dissent, for the following reasons :

1st. Repeated observation has convinced me, that we lose instead of gaining time by doing so. For if the eruption is suddenly checked by the exhibition of mercury, cold, or any other cause, it will most probably return again and again in successive crops ; a circumstance, of which Doctor Colles must

himself have met frequent instances, and which, I recollect, we witnessed more than once together in consultation, upon cases which *he* considered, at the time, very harassing and perplexing, as the patient had previously taken the *specific* in great abundance.

2ndly. In suppressing the eruption suddenly by the exhibition of mercury, the patient is much more liable to nodes and affections of the deep-seated parts, than if a contrary practice had been followed; a fact which is strongly supported by the published reports of the surgeons of the British Army, on the non-mercurial treatment, by which it appears that nodes are much less frequent under the non-mercurial treatment;* and for these reasons I dissent altogether from the four conclusions which the author lays before his reader at p. 226, in favour of the exhibition of mercury during the eruptive fever.

The cases detailed as illustrating the beneficial effects of mercury under those circumstances cannot be considered as affording any support to his position, for before the small doses given could affect the system, the eruptive fever had most probably declined. Our author himself seems, from the following passage, somewhat aware of the injurious effects of mercury during the eruptive fever:—"These two febrile states, viz. that of hectic and exhaustion, and that of the eruptive fever, require that the mercury should be administered in a manner peculiarly suited to such conditions. Were we to use mercury with these as we do with venereal patients in general, I believe we should commit most serious mischief. In these cases we should not commence with a larger dose than ten gr. of ungt. hydr. fort. every morning, or with an equivalent of blue pill; that is about gr. iij. mane nocteque." p. 234.

* Connected with these observations, it is worth mentioning, that so early as 1819, the Army Medical Board, in its report on the non-mercurial treatment, states, that "Where the primary symptoms have been treated with mercury, the secondary symptoms are more severe, and more intractable, than where mercury had not been used for the primary sores."

And in the next page we find him stating that three or four drachms of ointment are sufficient to produce the most salutary effects ; for not only, he observes, “ are the secondary symptoms dispersed by this small quantity, but the general health is proportionally improved, so that the patient rapidly acquires health and strength.”

It is amusing to find that M. Divergie, chief surgeon of the Gros-Caillou, an advocate for the anti-mercurial treatment exhibited the same minute doses of mercury as our author, but altogether under another motive. M. Divergie gave them *in finesse*, to satisfy such patients as imagined they could not be cured without that remedy : not that he had himself the slightest faith that such doses would be of any advantage ; but it is better to let him speak for himself.

“ When at the close of 1814, I became attached as senior surgeon to the Val de Grace, I was very desirous to make trials for arriving at the same result in the cure of recent venereal symptoms, by rejecting the employment of mercurial remedies. The principles explained in the works then published on the subject, and so accordant with the facts observed by myself, were assurances that I should succeed. But more than one obstacle prevented me from making conveniently, in the treatment of the venereal cases, the trials for which I was so anxious. Here as every where else, mercury under three forms, ointment, liquor, and pills, formed in general the treatment of the primary affections. Physicians and patients showed themselves equally persuaded that this metal was the only remedy against these distempers. To propose publicly a reform would have been equivalent to provoking an anathema against me. I was therefore obliged to proceed with reserve.

“ Nevertheless, from 1819 to the month of April, 1835, when I quitted the division of the venereal cases, aided by most of the pupils, I had several opportunities of introducing freely in the treatment of this class of patients, the improvements suggested by experience and reasoning. Those who, already vic-

tims of the effects of mercury, refused to take more, when symptoms recently contracted brought them again to the hospital, fell naturally under my care. *As to those who believed that mercury was the true specific, I began mercurial frictions in small doses, one drachm every two or three days, after the acute stage was over.* Already sparingly nutritive regimen, local blood-lettings, and rest had effected an important modification. Time passed; the cure advanced; and *seven or eight drachms* of mercurial ointment were sufficient to put these soldiers in a condition to quit the hospital after an abode of *thirty or thirty-five days*. Those, on the contrary, who were subjected to the ordinary mercurial treatment, used from *four to five* ounces of ointment, without counting the pills of Belloste (Ratier's Formulary, p. 123) taken in the interval between the frictions, and left the hospital only after fifty-five or sixty days. When the patients suffered themselves to be guided without anxiety as to the mode of treatment, I suppressed altogether the mercurial preparations; and *I arrived nevertheless at the same end; that is, of curing the patient more rapidly than by the ordinary method.* Instructed by so many observations agreeing with those afforded by my civil practice, I no longer doubted that *recent syphilis* might also be cured by simple and rational treatment. From this time mercury performed only a secondary part in my practice in the city. I soon renounced it almost entirely, and treated my patients by the antiphlogistic method.”*

The remainder of M. Divergie's communication consists in giving the result of his anti-mercurial practice from 1814 to 1835, and is chiefly composed of tables of the various classes of venereal symptoms thus cured, amounting to several thousands; to which I beg to refer the reader as a most important document. The difference between M. Divergie and Doctor Colles, in exhibiting these minute doses of mercury, appears to be this—

* Ed. Med. and Surg. Journal, from Archives Générales, October, 1835.

that the former deceived his patients for their advantage—the latter deceives himself to the advantage of no one.

In a work just published by Doctor Oppenheim, “On the Treatment of the Venereal Disease without Mercury, together with a notice of the antiphlogistic treatment of the disease pursued in the General Hospital at Hamburgh,” we find that the author is a decided advocate for this mode of treatment, and was amongst the first who introduced it into the German hospitals.

“He believes that it can be shown from historical data, that since the period when mercury became to be universally esteemed the proper remedy for the venereal disease, the latter has increased in the obstinacy and violence of its symptoms; and he maintains, that when the treatment of it by antiphlogistic remedies alone shall be generally adopted, the beneficial effects produced upon the present as well as the future generation, will be scarcely less than those produced from the discovery of vaccination!

“Notwithstanding the foregoing facts and observations (all relating to the antiphlogistic mode of treatment) it has been almost universally believed that syphilis can be effectually cured only by specifics, at the head of which was ranked, and still is ranked by many, mercury. That numerous other remedies and plans of treatment have, at different periods, been proposed, will be seen from the previous chapters of this work. *Diet, depletion, and abstinence, have, indeed, always been considered as necessary adjuvants to insure even the efficacy of their presumed specific.*”

For this notice of Doctor Oppenheim's work, we are indebted to the American Journal of Medical Science, as it has not been as yet translated into the English language. From it we find a host of evidence in addition to that afforded by the surgeons of the British army, in favour of the antiphlogistic mode of treatment; and I would beg particularly to call the attention of Doctor Colles to the shrewd observations which I have marked

in italics. I have now quoted the opinions and practice, recently published, of two most influential personages, the one in France and the other in Germany, directly opposed to those of our author, whose great object seems to be, to bring us back to the old orthodox faith—that there is no cure for a venereal disease but mercury; and that those modern heresies which have led some foolish people to recommend any other means, have induced incalculable mischief upon society. Now although it seems, from various passages, that our author's faith remains unshaken in the "*specific*" powers of this mineral, yet he has discovered that one-sixth, one-twelfth, nay a twenty-fourth part of the doses he was formerly in the habit of prescribing, will prove as effectual a remedy as the original quantum; thus we find in many cases detailed, particularly of the phagedænic venereal disease, characterized by rupia, that ten grains of the ointment rubbed in every night, or every second night, or three grains of blue pill every night, or every second night, will be sufficient to cure the disease. I feel no objection to allow our author to ride his hobby as hard as he pleases, provided it produces no worse results than this practice. I know how strongly he is wedded to early opinions and associations, and that I might as well attempt to shake a pious Musulman from his faith, that there is no God but Allah, and that Mahomet is his prophet, as to endeavour to dissuade my friend from his belief in the infallible powers of mercury over every form and every stage of venereal complaints—a belief which may aptly be parodied by a doctrine, the orthodoxy of which he will not dispute, that 'to overcome these maladies, there is no God but Mercury, and Abraham is his prophet.

In Chapter XI., "on the Treatment of Syphilis in Scrofulous Patients," I find that Dr. Colles still perseveres in the notion, that those enlarged cervical glands which occur for the first time, with the other symptoms of secondary venereal, are scrofulous. In my work on the former disease, I pointed out their occurrence as analogous and similar to that which takes place

in other exanthemata; for all the different forms of the venereal appertain to this class of diseases. He asks, when these glands appear, "how are we to treat this complicated case?" and then he thus replies to himself: "my answer is, that we are to proceed exactly in the same manner as if the venereal symptoms were uncombined with any such affection;"—and my answer would be precisely the same, although I do not consider the case to be at all complicated—the swelling of the cervical glands having arisen from the venereal poison, and not from any scrofulous diathesis. But in a truly scrofulous subject, with a predisposition to tubercular phthisis, Hunter, in my opinion, was perfectly right in deprecating the use of mercury, particularly when given, as our author recommends in such cases, so as to excite a smart ptyalism. Several instances are adduced, however, of the success of the mercurial practice; but I rather imagine that the enlarged glands had, in those successful cases, a venereal origin.

Chapter XV. is dedicated to the consideration of the non-mercurial treatment of syphilis; of course our author is not likely to approve of it, but allows that "fewer of the non-mercurial patients complained of affections of the bones, than those who had been ineffectually treated by mercury." Such an admission from Dr. Colles cannot but be esteemed of value. He also "acknowledges, that the profession is highly indebted to those who have lately introduced the non-mercurial plan of treatment, for we have not only acquired a second line of treatment for venereal cases, but, what is of the highest value, we have been released from an inveterate and deep-rooted error—from an unfounded conviction that the venereal disease could not be cured by the innate powers of the system, unless aided by mercury. I need not add, that all the opinions and practices consequent on this prejudice have been subverted."

Now, I agree perfectly with him as to the obligations which we owe to non-mercurialists; but I differ nearly as much in theory and practice from this class of practitioners, as I do from

the downright mercurialist. I look upon mercury as a most useful medicine for particular forms and particular stages of venereal diseases according to the system laid down in my work, which I have now pursued, both in private and public practice, upwards of twenty-five years, and never had occasion to regret my adoption of it. But the decided non-mercurialist, in his faults of omission, is perhaps equally erroneous, though not equally injurious to society as the decided mercurialist, who with one fell swoop, and without compunction, blindly consigns every venereal case to his powerful, and when thus indiscriminately employed, destructive *specific*.

At the conclusion of Dr. Colles's work, is an interesting chapter "on the Use of Mercury in Affections of the Nervous System," which he commences with the following observation: "Every surgeon who has been engaged in the practice of his profession during the last twenty years, cannot fail to have remarked the following fact; viz. that, during that period, mercury has been applied much more frequently to the cure of certain diseases, than it formerly was, although its powers over these had not been previously acknowledged. How to account for the increasing partiality to this medicine, for the cure of other diseases, while its use in venereal complaints, for which it had so long been considered a specific, has within the same period of time been by many practitioners objected to, and by some totally abandoned, is a problem not easily to be solved." He then conjectures that the general application of this medicine to diseases not venereal, has been owing to the salutary effects which it evinced in the treatment of the Walcheren fever. I should rather attribute it to the well known beneficial effects of mercury upon inflammation of the iris, for the first intimation of which we are indebted to Dr. Farre, in his letter to the late much lamented J. C. Saunders, on the disorganizing effects of mercury, published upwards of twenty-five years ago. It was this first opened my eyes to the benefits likely to result from its application in all inflammatory complaints, and from that period

I have been in the habit of employing it wherever I suspected the existence of internal inflammation.

In peritonitis, pleuritis, meningitis, periostitis, and lastly, in synovitis, its great utility is firmly established. In the last, or inflammation of the joints, my friend Dr. O'Beirne has put beyond a doubt, the vast advantages which result from a quick mercurialization of the system, in a paper inserted in the 5th volume of the Dublin Medical and Surgical Journal, and in the 4th vol. of the same publication, for September, 1833, I inserted a paper on "*Inflammatory Affections of the Brain and its Membranes*," containing six remarkable cases of head affections, including apoplexy and paralysis, five of which were treated successfully by bleeding, *mercurializing the system, and the counter-stimulus of tartar emetic ointment to the head*. The unsuccessful case, the first detailed, would probably also have had a fortunate termination, were it not for the wilfulness of the patient, who discontinued the medicine and my attendance, at two distinct intervals, on finding, as he thought, sufficient relief and amendment from the means employed.

Amongst observations I made at the time on the cases detailed, is the following:—"In the treatment of inflammatory affections of the brain and its membranes, next to blood-letting, in efficacy, stands, in my opinion, mercurialization of the system. The beneficial effects of this process in stopping the progress of inflammation of membranous parts, is most satisfactorily demonstrated every day by the exhibition of mercury for iritis. Frequently, in this affection, the pain, change of colour, and deposition of lymph on the iris, occasioned by the inflammation, begin to disappear, even before the mercury has had time to evince its usual effects on the gums of the patient."

Since that publication, I have met with a considerable number of cases of apoplexy, recent epilepsy, and paralysis, which have established, in my opinion, the value of this mode of treatment. In inflammation of the brain or its meninges, from

accident, it affords our best safeguard in protecting the patient against the formation of matter and its other consequences ; and this mode of treating all accidents of the head, in which internal inflammation is suspected, has been acted on with such success in the Richmond Hospital during the last four years, as to insure a continuation of this practice. I have this moment, in conjunction with Mr. Kavanagh, under my care a young lady residing at Kingstown, who has recovered by this treatment from an attack of paraplegia, by which she had totally lost all power over the sphincter of the bladder and the lower extremities. This young lady had been previously subject to most intense head-aches, which sufficiently indicated the seat of the disease.

I therefore hailed with pleasure the confirmation of my opinions and practice contained in this section of Dr. Colles's work, in which is detailed some remarkable cases of hemiplegia, of confusion of the intellect, with tendency to apoplexy, gastrodynia, and epilepsy, treated successfully with mercury and tartarized antimonial ointment.

But notwithstanding this general adoption of mercury, for inflammation of membranous and parenchymatous tissues, I have been informed by some eminent apothecaries of this city, of long standing, that on a rough calculation there is not a tenth of the quantity of that medicine ordered now, compared to its employment twenty years ago ; a fact which demonstrates the great diminution of mercury in the treatment of venereal complaints. Another fact is, I believe, also admitted by the seniors of the surgical profession, viz. that we do not now meet in the same proportion as formerly, extreme cases of broken down constitutions from the combined effects of venereal and its presumed antidote. We have, therefore, reason to conclude that the treatment of the disease is, by the majority of practitioners, better understood than it was twenty years ago ; but it must be admitted that in general practice there is still great room for improvement.

Notwithstanding that Dr. Colles and I disagree upon many points, both in theory and practice, respecting venereal complaints, yet there are several upon which I give my unqualified assent. For instance, I agree with him, contrary to the opinion of Hunter, "that constitutional symptoms are capable of infecting and communicating the disease." In addition to the testimony adduced by Dr. Colles on this head, I have met with several instances of new married women, whose moral character set them above suspicion, who received the disease from their husbands, who at the time of connexion had no primary, but had secondary symptoms; and in no other way could I account for children before the age of puberty becoming diseased in consequence of lying in the same beds with persons labouring under constitutional eruptions or ulceration.

His rules for conducting a mercurial course where such is advisable, are excellent, and his mode of fumigating by mercurial candles ingenious and suitable.

The chapter which treats of the venereal disease in infants, also contains much novel, useful, and interesting information.

His mode of treating a chancre at the orifice of the urethra with a strong escharotic, such as the white muriate of antimony, is decisive and most useful, particularly when the ulcer is of a phagedænic character; and when stricture of the orifice occurs after cicatrization of the ulcer, his peculiar mode of treating the stricture, as detailed at p. 95, is ingenious, and, I make no doubt, effectual. I also perfectly agree with him, contrary to the opinion of Hunter, that chancres sometimes occur in the urethra; but this was proved long since by the experiments detailed by Benjamin Bell.

Chapter VI. is on a "*Disease of the Lymphatic Glands of the Groin attended with peculiar Symptoms.*" In this affection one or more of the lymphatic glands of the lower or femoral range are generally the seat of the disease, of which I have met with in many instances. It is always attendant upon a delicate constitution, and rapid pulse. In every case of this de-

scription, I have been led to suspect the existence, or at least a strong tendency to tubercular phthisis ; and always directed my patients to the sea-side, and to observe those attentions calculated to improve the general health : and I perfectly agree with the author, that mercury in such a case would be injudicious and most probably destructive.—On the whole, I am happy in stating my opinion, that this treatise contains many useful practical lessons ; and that there is no surgeon, however extensive his practice may have been, who will not be benefited by a perusal of a work obviously the production of a man of extensive experience and undoubted talent for observation.

ART. IV.—*Remarks on Convalescence after Natural Labour.*

By FLEETWOOD CHURCHILL, M. D., Physician to the Western Lying-in Hospital, Lecturer on Midwifery, &c.

It may be necessary to offer some apology for occupying the attention of the reader with a subject so common-place as recovery from natural labour, were it not universally acknowledged that mankind are most apt to overlook, that which is most constantly before them. The subject has not occupied very much of the attention of those who have published systematic works upon midwifery, although it is certainly worthy of it, for it is within every one's experience, to be kept in a state of great anxiety about the recovery of a patient, who passed through the process of parturition in the most favourable manner possible. Moreover it not unfrequently happens, that certain symptoms arise, the initiative apparently of some serious malady, and which yet disappear without active treatment, and are followed by no grave consequences.

Quite recently, the ordinary phenomena of the puerperal state have been fully discussed, and the appropriate management pointed out by my respected preceptor, Dr. Hamilton of

Edinburgh, in the second part of his *Practical Observations*,* which I did not receive until after the rough copy of this paper was written. I have consequently rescinded, or but slightly referred to, whatever has been so much more ably treated by the learned Professor; and I have confined myself to noticing the *succession* in which the changes subsequent to parturition occur, and the *variations from the usual progress* of convalescence, limiting my observations to such variations as are not the precursors of organic disease.

If we examine a female *after* delivery, and compare her state with what it was *before*, even during an interval of pain, we cannot fail to be struck with the great change which has taken place. The act of expelling the child and secundines, seems to be attended by a certain shock to the nervous system, producing far more marked effects upon the constitution, than could have been caused by the mere muscular exertion. The skin becomes pale, flabby, and moist, the moisture having a peculiar odour; the muscles feel softer; the patient complains of exhaustion rather than fatigue; the organs of sense are unusually susceptible, there being more or less intolerance of light and sound, and a tendency to headach. The respiration is in some cases hurried, in others rather slow and deep, soon after delivery; the functions of the stomach are suspended; there is no appetite, but some thirst, and the action of the intestines is retarded. These symptoms are more or less observable in all cases, even the most favourable, and where no hemorrhage has occurred. If nothing interfere, the nervous system gradually recovers its tone, and in no respect is that restoration more manifest, than in its influence upon the various organic functions and secretions. The skin assumes a healthy look and feel; the appetite returns; and the bowels are evacuated naturally. The respiration resumes its accordance with the circulation;

* On the Ordinary Management of Women after Delivery, p. 1.

and so far the patient may be considered convalescent. There are some points, however, worthy of a somewhat more detailed examination, as for instance, the circulation, the state of the uterus, and mammary glands, &c. My data for the statements which are about to be made, are certain tables which have been accurately kept, at the Western Lying-in Hospital, by my very intelligent pupil, Mr. Gibbon.

If the pulse be counted just before delivery, but during an interval of pain, it will generally be found very frequent, almost always above 100, sometimes 140. If it be numbered an hour or so after delivery, it will be found to have fallen below the natural standard, say to 60 ; and if we examine again after ten or twelve hours, we shall find that reaction has taken place, and that the pulse is now quicker than when the patient is in health, but not so quick as it was just previous to delivery.

The amount of the primary vascular excitement, the degree of collapse, and the extent of the reaction, depend partly upon the character of the labour, partly upon the irritability of the patient's constitution, and partly upon causes which are often very obscure, and which it would be foreign to my purpose to investigate at present. But in almost all the cases I have on record, there was a decided relation between the three states ; *i. e.* supposing the pulse of the patient to have been very high before the birth of the child, it generally fell very low immediately afterwards, and rose again proportionally in the course of a few hours ; and *vice versa*, when the primary excitement was slight, the collapse and succeeding reaction were trivial. Variations from this, the ordinary course, I shall have occasion to notice by-and-by. A considerable acceleration of pulse, with rigors, is observed when the secretion of milk commences, but this gradually subsides, and the circulation becomes quiet and regular. All the observations I have been able to make, confirm the accuracy of WILLIAM HUNTER's remark, that no female, after delivery, can be considered safe, whose pulse is not under 100.

Immediately after delivery, the uterus contracting, may be felt at the lower part of the abdomen, about the size it was at the fifth and sixth month of pregnancy. It feels hard and firm, and is scarcely at all tender on pressure, except when the after-pains are severe. Its bulk is gradually diminished by successive contractions and, according to Dr. Hamilton, by absorption,* and about the eighth or ninth day, it is within the bony pelvis. The after pains occur in paroxysms with distinct intervals, just like labour pains; on the occurrence of each, the womb becomes hard, is tilted forward, and expels any clots or blood which may have been retained in its cavity.

From the period of delivery there is a constant discharge from the womb, lasting two, three, or four weeks, and even longer in delicate females. On the *first* day this appears to be pure blood, partly fluid and partly in clots; the *second* day it is much thinner, and paler, rather a secretion or an infiltration than a discharge from open vessels; on the *third* day it becomes more or less slimy, still paler in colour, and generally less in quantity: by the *sixth* or *seventh* day, the colour changes to a dirty yellow or greenish hue, after which it gradually becomes colourless, opaque, or transparent, and diminishes in quantity until it entirely ceases. It possesses a very peculiar odour, which is readily recognized in the atmosphere of the wards of a lying-in hospital. Dr. Lowder compared it to the smell of fish oil.

Before delivery, the mammary glands generally secrete a thin, yellowish fluid, very different from milk; but cases are not unfrequently met with, where proper milk is secreted in such abundance, as to enable the female to give suck to the child immediately after its birth; and for children so supplied, medicine has been found unnecessary. I have further remarked that such females generally suffer severely from after pains, each application of the child to the breast bringing on immediately a fresh paroxysm.

* See the Essay before quoted, p. 7.

The secretion of milk before delivery is not the ordinary occurrence, however; for many hours afterwards the mammary sympathy is not excited, but after an interval of from twenty-four to fifty hours some uneasiness is felt in the breasts, with occasional darting pains, and an augmentation of volume. As yet milk is not produced, but the glandular substance feels hard and knotty, the areola is puffy, and the nipple prominent, as it were, erect. In the course of the next eight or ten hours, the size and weight of the breasts increase very much, and they feel hard and tense. If the child be applied, he will probably be able to obtain milk, but less freely than after the lapse of a short time, and a little more practice. The flow of milk affords great relief to the patient, it diminishes the local distress, lowers the pulse, and lessens the heat of skin, which characterize these few days of milk fever.

These are the principal phenomena which follow natural labour, with their successive changes. It now only remains to point out certain variations from this regular course, of sufficient magnitude to demand our attention. And first, of the *shock inflicted upon the nervous system*, and thence upon the system generally. I have already said, that I do not believe this to be the result of the muscular exertion merely, and whatever doubt of this being the fact may be entertained in the more favourable cases, none can exist as to the severer ones. A due estimate of the nervous shock is of great importance, for in almost every instance (when serious disease is not in question) the recovery of the patient is in inverse proportion to the amount of this disturbance. There are different degrees of suffering resulting from it, the more favourable cases exhibit very little, in others it is very marked, and in these we may observe (in addition to the ordinary symptoms already enumerated) a sunken countenance, laboured, hurried, or panting respiration, great depression, restlessness, small, quick, fluttering pulse; and I have very often observed that the proportion between the respiration and circulation is destroyed. In an

extreme case, if the constitution be feeble, the patient may sink in the course of a few hours, before inflammation has had time to set in, and if a *post mortem* examination be made, no lesion will be found sufficient to account for the death of the patient. In one such case, I found the uterus, ovaries, and vagina, all uninjured and free from disease, nor was there any lesion of any other organ. The patient had sunk, as I have described, within a few hours after delivery. But these are extreme cases, and fortunately not very common, being chiefly those in which the patient has been neglected, or when assistance has been too long deferred.

The length of time which may elapse before a patient arrives at the *maximum* of susceptibility to the nervous shock, varies much in different individuals, dependent probably upon constitutional peculiarities. Labour may go on very long with strong women, and yet the shock be very moderate; with others, on the contrary, a few hours of protracted natural labour involves as much danger as an operation. A third class of patients cause great anxiety to the medical attendant, they go on for a long time so well, and so free from constitutional suffering, that no mischief is anticipated, yet beyond a certain point, their power of endurance is exhausted, and they "run down" with great rapidity.

There are many cases, however, where the shock received, though far from being as severe as in some which have been described, is yet quite sufficiently so to excite uneasiness and even alarm in the medical attendant. Instead of the proportionate collapse and re-action which ought to succeed delivery, we find the patient exhausted, panting, and much distressed, with a quick pulse. Re-action is long before it occurs, or it may take place imperfectly or excessively, and the patient generally experiences an unusual degree of weakness, with a tedious convalescence. It would be out of place to dwell much upon *treatment*, considering the excellent directions given by Dr. Hamilton, but I may be allowed to remark *en passant*, that

the most *obvious* remedy for this state of nervous depression is not the *best*. The exhibition of stimulants (as wine, brandy, &c.) to any considerable extent aggravates the condition of the patient, instead of relieving the collapse, and in very moderate quantity only are they admissible. A good dose of opium, or small doses repeated pretty often, will afford more relief; it not only gives the patient a chance of sleep, the best restorative of all, but even if it fail in this, the system will be quieted, the respiration rendered more equable, the pulse slower and more natural, and the balance between these two systems restored. After a time, it may be necessary to give some tonic medicine, and I think musk will be found one of the most useful. The diet of the patient must be carefully adapted to the enfeebled condition of the digestive powers.

To return.—The second point mentioned was the state of the circulation, with the alternation of excitement, collapse, and reaction. I have just stated one variation from this succession, in the case of a severe nervous shock, when the pulse may continue rapid after delivery, instead of falling. I have often remarked an undue frequency of pulse when the after-pains are violent, and as the uterus is also somewhat tender on pressure in such cases, it requires tact and care to distinguish between this state and the commencement of puerperal fever. The same observation will apply to the quickening of the circulation which takes place when lactation commences, and which, in addition, is accompanied by rigors. A careful estimate of all the symptoms in either case will generally elucidate the nature of the excitement, and our observation of the diminution instead of the increase of vascular action will decide the question. Again, in cases where a coagulum is retained in the uterus, the pulse is quickened. I had noticed this repeatedly before I could discover the cause, but having found it subside immediately on the discharge of clots, I have no doubt that this was the cause.

Lastly, the pulse will be greatly accelerated, if the patient suffer from diarrhoea or gastric disturbance; and as it is not

very easy to foresee the issue of such an attack, the utmost watchfulness will be required. The diagnosis may be very obscure, and it may be necessary to adopt certain measures rather suited to the attack we fear, than to the disturbance from which the patient is suffering. Along with the soothing and astringent medicines adapted to the state of the bowels, it will not be amiss to administer small doses of blue pill or calomel, in combination with opium.

With regard to the variations from the ordinary size of the womb and its gradual decrease; I have found sometimes, on the fourth or fifth day, that its bulk had *increased*, and that it felt less firm than previously; this, combined with the quick pulse, has made me fear an attack of hysteritis, nor was this fear diminished by the uncomfortable sensations of the patient, or by the fact, that in some cases the lochia had suddenly diminished in quantity. Upon applying fomentations of hot water or turpentine to the abdomen, a quantity of coagula were discharged, and the patient obtained speedy relief. Purgative enemata also favour the expulsion of the clots, and in such cases have been given with great benefit.

It has been already mentioned, that the uterus is not free from tenderness in those cases where the after-pains are severe, and if it be rudely pressed, the outcry of the patient may mislead us. It will be observed, however, that this tenderness is greatest *during each uterine contraction, and that as these subside, it diminishes*. Fomentations to the abdomen will generally mitigate this sensibility, but if the after-pains be severe, and the tenderness considerable, a full dose of laudanum, followed by an aromatic purgative, will probably relieve both.

Perhaps no phenomena in the progress of convalescence excite more alarm in the patient's mind, or show more the value of minute observation on the part of the accoucheur, than the variations in the quantity, quality, and odour of the lochia, which sometimes occur. The patient will hardly be persuaded that such are not the unfailing indicia of organic disease. Yet very remarkable changes do occur without any lesion of the

uterus or vagina. In some cases, the lochia after decreasing for some time, are suddenly discharged in double quantity, and of a much brighter colour, but without coagula. This generally happens when the patient is permitted to sit up too soon. Or it may happen at a later period, in consequence of walking about too much. A little extra rest will, however, suffice to restore the patient to her former state.

Again, the os uteri is sometimes obstructed by a coagulum, and the lochia are greatly diminished, or perhaps entirely retained, until the expulsion of the clot affords an exit to the accumulation.

It has been already stated, that the lochia generally continue three or four weeks, according to the constitution of the patient, but sometimes for six or eight, and then terminate in persistent uterine leucorrhœa. This will best be remedied by counter-irritation to the sacrum, and the internal exhibition of copaiba, iron, or ergot of rye.

In connexion with this subject, I may just mention, that the menses frequently return the first or second period after delivery, without again recurring during lactation. This would appear to be most common after the birth of first children.

Again, the quantity of the lochia being unaffected, the colour may excite alarm; instead of the transition from red, to a pale red, yellowish, or greenish colour, the lochia are sometimes of a dark brown, and perhaps more tenacious than usual, or they may suddenly become perfectly colourless, (resembling the discharge called whites,) but in neither case attended with inconvenience or danger. It is very necessary to be on our guard, when the lochia become of a redder colour than, from the period which has elapsed after delivery, they ought to be, as this change may be the precursor of secondary hemorrhage. The patient should be confined to the horizontal position, and clothed very lightly.

I have met with two or three instances, where the lochia had a very offensive odour, without any other evidence of dis-

order, local or general. There was neither hysteritis, nor sloughing of the vagina. The patients recovered perfectly. In such cases, it is advisable to wash out the vagina once or twice a day with warm water, to prevent any irritation from the offensive discharge.

Variations in the period of the secretion of milk, are of no moment; if the vascular action be excessive, it must be moderated by antiphlogistic remedies; such as tartar emetic, fomentations, &c., and by the frequent application of the child. If, as in some very rare cases, no secretion should take place, the child will require a wet nurse, but the mother will not suffer.

Many more variations from the ordinary course of convalescence after natural labour might be added, but I have been rather anxious to confine myself to those which are the most marked. I am far from presuming to suppose, that these changes have not been noticed by all who have seen much practice, but for the younger members of the profession these observations may not be altogether useless. I have expressly omitted the details of the management of the puerperal state, because, I can refer the reader to Dr. Hamilton's work, where all I could say is stated more lucidly, and on far better authority.

ART. V.—*A Case of Disease of the Spino-occipital Articulation, with the Post Mortem Appearances.* By TRAVERS R. BLACKLEY, M. R. C. S., &c.

MARY KING, aged eight years, was admitted March 17th, 1837, into the Hospital for Children, Portobello, for violent headaches, to which she had been very subject for the last two months; these pains commenced suddenly on her return in the evening from the funeral of her sister, to whom she was much attached. There was an appearance about this patient which convinced me that the cephalalgia was to be attributed to the existence of some decided organic disease.

Her countenance was peculiarly expressive of caution, and was florid, and full, if not bloated ; the chin was advanced preternaturally beyond the chest, the mouth slightly opened, and she kept the arms parted from the sides as if to poise herself. On looking laterally, she strained her eyes in the direction of the object, and failing in this, turned her entire body for the purpose. The effect produced when she attempted to observe anything placed near her feet was yet more remarkable ; for this purpose she generally put her hand to her forehead, as if fearful of undue weight in the head, and bent her body, thus avoiding the least motion between the first and second vertebræ. In getting up from bed also, or in lying down, she invariably supported the head with her hand.

From a consideration of these symptoms, I did not hesitate to attribute the disease to the cervical vertebræ. All the inferior ones appeared sound ;* and when I took into consideration the very important motions performed by the atlas and second vertebra, which in this case were extensively impaired, if not totally destroyed, and the organization necessary for the performance of these motions, I at once referred the seat of her complaint to these latter parts.†

The headach was not permanent, sometimes she was free from pain for a couple of days, especially while using the warm bath, and on those occasions exhibited a most cheerful disposition ; as the disease advanced, however, the intervals of rest were fewer and of shorter duration ; the use of calomel seemed to afford considerable ease, until it slightly affected the mouth on the fifth day, from which time her sufferings appeared rather to be aggravated ; leeching and blistering, which had

* She exhibited no pain whatever on pressure over any part of the spinal column, even close to the occiput.

† Two months before this child died, I expressed my apprehension lest she should expire suddenly, upon her head being raised, as the transverse ligament might give way ; an accident which occurred some years ago at the Richmond Hospital.

been tried at the commencement of my treatment, to the back of her neck, were perfectly useless, nor was the insertion of a seton in the same situation attended by any happier result.

The sufferings of this poor little creature were really most painful to witness, and at times amounted to agony ; on these occasions she always wished to have a bandage bound tightly round her forehead, or requested the nurse to squeeze her head between her hands, but the most effectual remedy was the tincture of opium in small quantities, repeated when necessary.

Of late she referred the headach more to the left than the right side ; the pain on several occasions left the head, and traversed the spine to the sacrum, from whence it returned to its original seat ; she also complained occasionally of pain in her stomach, but this was always relieved by the application of a jar of hot water to the part ; the bowels were regular throughout.

From the 14th of May she was unable to raise herself in bed, and her appetite completely failed, but at times she drank copiously ; she spoke very little, and her emaciation became extreme ; during the last four days of her life the right arm was powerless, but slight grasping motions were observable in her fingers ; there was great lachrymation from the left eye, and it was doubtful whether she could distinguish objects with it. She expired without a struggle on the 29th June. Owing to the objections entertained by her friends to have the body examined, I was obliged to content myself with the removal of the diseased portions, namely, the cervical portion of the spine, and the portion of occipital bone in connexion with it ; and with my friend Dr. Houston made a minute examination, the result of which was as follows :

The articulating surfaces between the first and second vertebræ, and between the condyles of the atlas and occipital bone, were diseased, the cartilages eroded, the denuded bones gritty, and the capsules thickened and coated with lymph. The capsules of the loose articulations between the oblique processes of the atlas and dentata were particularly enlarged, and pro-

truded forwards under the deep muscles of the spine, forming three distinct abscesses pressing against the pharynx; that on the right side had made its way downwards for more than an inch between the bones and muscles. The odontoid process was completely detached on all sides, and stripped of cartilage and synovial membrane, it was immersed in pus. The perpendicular, oblique, and transverse ligaments had altogether disappeared, and the odontoid process was only separated from the spinal marrow by the thickened sheath of that organ. All the joints enumerated, including those of the odontoid process, appeared to be engaged in one suppurating surface.

The fibrous sheath of the medulla oblongata, and commencement of the spinal marrow, were much thickened in texture, and the arachnoid and pia mater to the same extent were red and swollen, but the roots of the nerves were all perfect, and the medullary texture did not exhibit any organic lesion. The latter was perhaps a little softened, but had not undergone any alteration of colour; I say a *little* softened, because, as Dr. Houston remarked, in the healthy state this part of the spinal marrow feels more resisting than any other, and in this instance it yielded most easily to pressure with the finger.

The entire, which has been most carefully prepared by Dr. Houston, may now be seen in the Museum of the College of Surgeons.

ART. VI.—*History of a Case of Extensive Carcinomatous Disease*. By ROBERT W. SMITH, A. M., M. R. I. A., Lecturer on Surgery at the Richmond Hospital School of Medicine, &c. &c.

CATHERINE WAKEFIELD, æt. 30, admitted into the Richmond Hospital, March 2nd, 1837, under the care of Mr. Adams, labouring under carcimona of the lymphatic glands in the right axilla. About twelve months previous to her admission, she noticed in the above-mentioned situation a small tumour, hard,

moveable, and free from pain. A few months afterwards the right shoulder became swollen, painful, and tender to the touch : a number of small, hard, pale tubercles developed themselves in the integuments upon the right side of the chest and neck ; and the pain and swelling of the shoulder extended down the arm and forearm : her general health now declined, and her complexion became sallow. At the time of her admission into the hospital, the mammary gland did not appear to be implicated in the disease ; but the posterior wall of the axilla was occupied at its lower part by a tumour of stony hardness, irregular and knotty upon its surface, and extremely tender to the touch ; and the integuments of the axilla and shoulder, the right side of the chest and neck, were thickly studded with the small tubercles already mentioned. Shortly after the patient was admitted, however, the breast itself became engaged ; the entire gland became the seat of a hard, painful swelling : the nipple was not at any time much retracted ; but the skin around it presented numerous little hard grains or tubercles. At length the integuments over the tumour in the axilla became livid, and finally ulcerated. The arm was seized with exquisite pain, and became œdematous to such a degree, that the pulse was not distinguishable in the radial, nor even in the brachial artery ; and towards the close of her life, the upper part of the arm became ecchymosed ; large vesications containing a dark serum formed, and gangrene evidently impended. About a fortnight after the woman was admitted, she was attacked with symptoms of gastritis ; vomiting of green fluid ; tenderness of the epigastrium ; desire for cold drinks, and inability to retain them for more than a few moments ; the tongue was red and dry ; the abdomen was slightly tender upon pressure ; the skin hot ; the feet cold ; the pulse 100, small and weak. This attack lasted for about a week, and was followed by a series of very anomalous symptoms. She ceased to complain of pain ; did not notice any thing that occurred about her ; in fact, consciousness appeared to have been lost ; the eyes were fixed ; the pupils

insensible to the stimulus of light ; and, as in catalepsy, the arm and hands would remain in any position in which they were placed. At this period of her illness, her bowels became obstinately constipated, and continued so for ten or twelve days ; at the end of which period, jaundice set in, and the alvine discharge presented the appearance usual in that disease. Shortly after the occurrence of the jaundice, she gradually sunk, and for a day or two before her death remained in a comatose condition.

The *post mortem* examination in this case, proved exceedingly interesting ; inasmuch as it very satisfactorily explained the greater number of the symptoms. Upon opening the chest, the right lung was found closely adherent throughout its whole extent to the side of the thorax : in every other respect the contents of that cavity were normal. Such, however, was not the case with regard to the abdominal and pelvic viscera : the peritoneum was inflamed, and here and there lymph had been effused. The ovaries (each of them nearly as large as an orange) were changed into a scirrhus structure of a yellowish white colour ; they compressed the rectum to such a degree that the little finger could scarcely be passed through it. The lining membrane of the intestine, below the compressed portion, was intensely vascular and inflamed : while above the ovarian tumours, it was remarkably pale and covered with the peculiar clay-coloured feculent matter, usually present in jaundice. The glands along the abdominal aorta were enlarged and indurated ; the gall bladder, distended with green bile, had passed down several inches below the edge of the liver ; the hepatic, cystic, and upper portion of the common duct were likewise greatly distended. The pancreas was slightly enlarged, and converted into a true scirrhus structure, in which the lobules of the gland could not be distinguished : the enlargement engaged chiefly its right extremity, which had surrounded and compressed the termination of the ductus communis to such a degree as to cause its almost total obliteration. The axillary and brachial

veins were dilated ; and the blood was firmly coagulated in them throughout the greater part of their extent : both roots of the median nerve, where the brachial artery passed between them, were enlarged and indurated. The tumours thus formed were oblong, and about one inch and a half in length, and must have compressed the artery to a certain extent. The breast presented the usual appearance of scirrhus of that organ, with the exception that the fibrous bands were not very well marked. The head was not examined, owing to what cause I do not at present recollect. It is much to be regretted, as in all probability disease would have been found, and some explanation afforded of the nervous symptom mentioned in the history of the case.

Notwithstanding, however, this deficiency, the case possesses interest in more than one respect. In the first place, it affords us an example of carcinoma commencing in the axillary glands ; the breast being only secondarily engaged : and I am inclined to believe that such cases are more rapid in their progress, and more fatal in their termination, than those in which the lymphatic disease succeeds to that of the breast ; for the former case seems to indicate a more general contamination of the system, and is probably more frequently combined with visceral disease. At all events, in the case of Wakefield, no hope of recovery could have been entertained, even before the development of the disease in the mammary gland ; for the integuments were thickly studded with those hard, pale tubercles, which, when present, so uniformly denote the inveterate nature of carcinoma. Operations undertaken in such cases are miserable in their consequences. I have seen them performed under such circumstances more than once ; but in every instance the disease returned before the wound was healed, and reached its fatal termination with frightful rapidity. To the intelligent surgeon, they are beacons, which warn him of the danger of interfering with the disease. The swelling, the great œdema, and threatened gangrene of the arm, are to be referred to the con-

dition of the glands and veins of the limb ; the structure of the former was completely altered, and the functions of both glands and veins destroyed : in fact, the circulation through the veins must have been arrested to a greater or less degree for a considerable time before death. I do not think that sufficient attention has been paid to the state of the circulation in these cases ; the veins should in every instance be examined, to ascertain whether or not, and how far they are concerned in producing the œdema of the arm, so commonly met with in cancer of the breast, and so uniformly referred to the obstruction of the lymphatic glands. In the case of Wakefield, even the arterial circulation must have been diminished, the artery being compressed by the diseased glands, and also by the tumours found upon each root of the median nerve.

The obstinate constipation which this woman laboured under for many days was owing to two causes, either of which would have been adequate to the effect : first, the intestines were no longer stimulated by the influx of bile, owing to the compression exercised upon the ductus communis choledochus by the pancreas ; and secondly, the contracted rectum could scarcely permit the passage of fecal matter, so much was it compressed upon either side by the enlarged and scirrhus ovaries. The cause of the jaundice is obvious and of a purely mechanical nature ; about an inch of the duct was completely imbedded in and surrounded by the indurated pancreas ; Anel's probe could not be passed through it without using force, and it required much time and patience to discover its opening into the duodenum. It is seldom we meet with disease of the pancreas ; we often find, as Roche and Sanson observe, in the bodies of those who die of cancer of the stomach or liver, the pancreas surrounded by cancerous masses, developed in the surrounding cellular tissue, while the gland itself is almost always sound in the midst of this disorganization. Jobert mentions a case in which a tumour, developed in the pancreas, caused the almost total obliteration of the duodenum ; and Mr. Todd has given the par-

ticulars of a case in which the pancreas was scirrhus. The distended state of the gall bladder was remarkable in the case of Wakefield, but not so much so as I have seen in other cases. I have very frequently found it forming a tumour sensible to manual examination ; and under such circumstances, it has been more than once mistaken for an abscess and punctured. Andral has seen it distended to such a degree, as to touch the crest of the ileum, and even descend before it as far as the iliac fossæ : but the most remarkable case of the kind is, perhaps, that published by Mr. Todd in the first volume of the Dublin Hospital Reports ; the case is so interesting, and bears so much upon that of Wakefield, that I trust I shall be excused for again bringing it before the profession ; the patient was a girl æt. 14. “ At the time I saw the girl,” says Mr. Todd, “ her skin was of a deep orange colour ; she was greatly emaciated ; had anasarca of the lower extremities, and appeared to suffer much pain, although almost wholly insensible to external objects. She was unable to speak, and seemed not to understand any question put to her. Her hands were firmly closed ; her jaws fixed, so that it was with difficulty I could pour a teaspoonful of wine into her mouth. She moaned incessantly, and frequently screamed, as if suddenly seized with acute pain. Upon examination, I discovered that the abdomen was distended with fluid, and that the epigastric and right hypochondriac regions were particularly tumified ; a distinct, tense swelling occupied these regions, and could be traced extending even below the umbilicus. At one point, which appeared more prominent than the rest, a little below the ensiform cartilage, and to the right of the linea alba, a fluctuation was very evident. This part was extremely sensible, as the least pressure on it appeared to increase the patient’s sufferings ; and I thought it very probable that a large abscess of the liver was here approaching the surface. With this impression, and anxious to afford immediate relief, I made an opening into the most prominent part of the tumour, by dissecting cautiously with a lancet, and gave exit to a thin fluid,

coloured with green bile. From the appearance of the fluid which escaped, it immediately occurred to me that I had mistaken an over-distended gall bladder for an abscess, and having opened it, that its contents had mixed with and coloured the fluid effused into the abdomen. Wishing to prevent as much as possible the further extravasation of bile into the cavity, I introduced the canula of a trocar through the wound, which fortunately passed into the sac ; and the opinion which I had formed was then strengthened by my observing that the fluid discharged through the canula was viscid, green bile, of which I drew off upwards of two quarts. After withdrawing the canula, a considerable quantity of the thin fluid was discharged, such as passed through the wound before the tube was introduced, and the tumefaction of the abdomen entirely subsided. I then carefully examined every part of the abdomen, but could not discover enlargement of the liver, or any other swelling ; the patient, however, exhibited signs of augmented pain, when pressure was made on the epigastric region. In the course of the day, the girl appeared in a slight degree relieved, and became more sensible ; but in the evening, all the unfavourable symptoms returned ; the belly became swollen, painful, and tense ; she had several attacks of convulsions during the night. Next morning she was completely comatose ; her respiration laborious ; her pulse weak and intermitting ; and she died in the evening of that day.

“ On the following morning I examined the body. When the abdomen was opened, a large quantity of serous fluid, mixed with green bile, was discharged ; the peritoneum was inflamed in several parts, and flakes of coagulable lymph adhered to its surface, and floated in the fluid. The liver was perfectly healthy, and of its natural size ; the gall bladder was empty, and contracted ; the hepatic and common ducts were enormously distended, and contained more than a quart of bile, exactly similar to that which passed through the canula in the operation. *These ducts formed a sac which extended from the porta*

of the liver to the os sacrum, lying behind the duodendum, pancreas, and root of the mesentery, and stretching in a transverse direction, so as to cover the anterior surface of the right kidney, and the greater part of the left. Having discovered and dilated the opening in the sac, which had been made with the lancet, I traced the distended duct up to the liver, where it received the smaller hepatic ducts, and these were so much enlarged, particularly that of the right lobe, as to admit of the introduction of one of my fingers without difficulty. The cystic duct, at its junction with the hepatic, was dilated for about half an inch; but on examining the interior of it at this part, a sort of convolution or fold seemed to have been formed, which probably acted as a valve, and prevented the bile from passing to the gall bladder; the remaining portion of this duct was pervious, and of its natural size. I next proceeded to search for the extremity of the common duct, which opens into the duodenum, but could not discover it; the pancreas was scirrhus, and that portion of the gland with which the ductus choledochus is connected, together with the surrounding cellular substance, and absorbent glands, was converted into a hard, solid mass, closely adhering to the duodenum and lower part of the enlarged biliary duct, which last seemed to have been completely obliterated. The pancreatic duct was also obliterated near to the intestine, and the biliary duct thickened, and its inner surface lined with a gritty deposit. Such is the history of this remarkable case; and it affords, as Mr. Todd remarks, a striking example of the power which some organs possess of accommodating themselves, both in their capacity and structure, to the force of distention when gradually applied to them. The history of another case, in which the gall bladder was punctured, has been published in the *Dublin Hospital Reports*, vol. v., and also in the *Cyclopædia of Practical Medicine*, by Dr. Stokes, in the article upon *Inflammation of the Liver*, to which I would refer for some interesting remarks upon obstructions of the biliary ducts.

ART. VII.—*Contributions to the Pathology of Glanders in the Human Subject.* By Dr. W. ECK.

[Translated from the Medicinische Zeitung for May, 1837, Nos. 18 and 19.]

THAT the introduction of contagious morbid products, derived from the lower animals, into the human system, is capable of producing much good and much evil, is universally known. The protective of vaccination, and the disastrous effects of inoculation with the hydrophobic virus, afford striking examples. The experience of modern times has taught us, that many contagious diseases of the lower animals exercise a pernicious influence on man; of these I may mention mortification of the spleen and its consequences, the black pock (*Schwarze Blatter*), and glanders, with its effects on man.

The phenomena of glanders (a disease attributed to lymphatic cachexy by Veith, and ranged among the tubercular affections, as a specific degeneration of the cellular tissue, by Dupuy) in its acute or chronic form are well known, and have been graphically described in the official reports on contagious diseases. We are also well acquainted with the phenomena of button farcy, a disease which bears a close relationship to glanders, frequently conjoined with it, and generally arising from one and the same source, occurring only in the genus *Equus* and its varieties, and distinguished chiefly from glanders by the affection of the cutaneous lymphatics, while in the latter disease those of the internal surface are engaged. It is likewise generally admitted, that glanders may become spontaneously developed in the horse by errors in the nutritive function, that is to say, under conditions which give rise to a morbid state of the lymphatic system, and particularly of those glands which have a more intimate organic connexion with the mucous membrane of the nostrils. Authors, however, are less generally agreed as to the power of infection which this disease possesses with respect to animals of the equine species. Even in the most

modern times, it has been expressly denied by Godine, Dupuy, and others. The inconvenience of many police regulations, founded on a conviction of the contagious nature of the disease, has favoured the partial introduction of the opposite opinion, which the uncertainty of the diagnosis in the first stage, and the fact, that a horse labouring under glanders, continues for such a length of time in the apparent enjoyment of general good health, has tended to corroborate. Again, where one horse labours under glanders, and a second or a third becomes affected, it is maintained, that each have been exposed to the influence of similar atmospheric or local causes; and where a horse exposed among a team of glandered horses, remains free from the disease, it is looked upon as a convincing proof of its non-contagious nature. On the other hand, the inoculations made by Viborg,* and his extensive experience of the destructive consequences of the foregoing views, have diffused more widely the conviction of the contagious nature of glanders. Besides the reports of almost every provincial board of health, in modern times, have furnished cases of the kind; and all our medical police councils have assumed as *proven* the contagious nature of glanders, a fact of which no experienced dealer in horses now entertains a doubt. And, though the fluid which flows from the nostrils is recognized as the chief source of infection; and that danger is chiefly to be apprehended from the use of utensils, mangers, and drinking vessels, &c. which have been in contact with glandered horses, and which are so likely to come in contact with the mucous membrane of the nostrils of sound horses; still, we can scarcely doubt, that the contagion may not also exist in other excretions; and that occasionally, as for instance, in close, damp stalls, sound horses may become affected without any contact, probably through the medium of the inspired air. That many horses are less susceptible of this disease; that the intensity of the disease itself differs in degree; that the matter of glanders loses its infectious properties by exposure to heat

* Über Potz, Wurm und Kropf der Pferde, in Seinen Sammlungen. Band. 2 and 3.

and to fresh air; and that sound horses may remain for months in company with glandered without taking the disease, provided a certain species of communication be prevented; these are circumstances which are analogous to those connected with other species of contagion, and do not militate against the supposition of their infectious powers. With regard to infection, glanders and button farcy preserve an identity in the dyskrasy to which they owe their origin in this point also,—that the latter is produced not alone by inoculation with the purulent matter generated in the ulcers, but by inserting the matter which flows from the nostrils of a glandered horse into the skin of a sound one; and *vice versa*, glanders is produced by inoculating the mucous membrane of the nostrils with matter taken from an ulcerated spot of button farcy.

The injurious and highly pernicious effects of glanders on the human subject is much less generally admitted. Up to a very recent period, only a few veterinary surgeons recognized the fact of its destructive properties in their published treatises, and even then, for the most part, with certain reservations. Thus, for instance, Professor Waldinger* says, “In dissecting horses who have died of glanders or button farcy, should the operator cut himself, great care must be taken not to let any pus get into the wound, as the most deplorable consequences, and even death itself, are to be apprehended.” From this it appears that the only dangers he dreads, are connected with the opening of bodies, and from pus getting into a wound. Director Veith† observes, “The specific effects of the contagion of glanders act solely on animals of the horse species, and operate on other domestic animals chiefly as an acrimonious animal fluid. In man,‡ inoculation with the matter of glanders (which generally occurs from a cut finger coming in contact with the matter, or from the matter getting into the eye when the animal

* Wahrnehmungen an Pferden. 2te. Aufl. Wien, 1810, p. 95.

† Handbuch der Veterinairkunde, 2te. Aufl. 1822, S. 685.

suddenly and forcibly expels it from the nostrils,) brings on violent inflammation of the parts, which is extremely painful and obstinate, involves by sympathy the neighbouring lymphatic glands, and bears a resemblance to the arthritic inflammations." From this it would seem that Professor Veith was merely acquainted with the injurious local effect of the poison on man. The late director of the veterinary school in this city, Professor Naumann, and Veterinary Surgeon Hallbach,* when asked their opinion as to the infectious power of glanders with respect to the human subject, in a suspicious case, which was under the care of Dr. Schilling in 1821, stated, "that they had not met with any example of the disease in man, from glandered matter received from a living horse, and that none of the veterinary surgeons or grooms who had been occupied with living or dead glandered horses, had ever been infected. On the other hand, however, there were not wanting instances in which persons who had cut themselves, while making preparations from such bodies, occasionally had bad forms of inflammation, and even mortification affecting the hands and forearms." About the same time also Veterinary Surgeon Major Dietrich† disputed the possibility of the human subject taking infection from a living horse, but admitted that he had met with some cases, in which men who cut themselves while dissecting the bodies of horses who died of marasmus and glanders, became ill, and got mortification of the injured part, which proved fatal. Nay, Surgeon Hallbach was so little afraid of infection from living horses, that he offered to inoculate himself with the matter of glanders.

The fact of the existence of such opinions among experienced veterinary surgeons, the few opportunities which most practitioners have of observing the disease in man, the ignorance of its symptoms among former observers, and the false representations which have been given of cases, will be sufficient to explain the prevalence of the foregoing views among the majo-

* Rust's Magazin, B. xi. S. 300.

† Rust's Magazin, B. xi. S. 510.

rity of medical men; and hence I have not been much surprised to hear, not long since, from medical functionaries, and practical physicians of the first class, similar opinions as to the unproved, or at least hypothetic and improbable noxious qualities of glanders with respect to man. Since 1821, however, a different opinion has prevailed, founded on the experience of hospital, military, provincial, and veterinary surgeons, and has been communicated to the public in official reports, dissertations, and widely circulated journals. Among these I may mention the interesting case of an artillery man, named Rennspiess, detailed by Dr. Schilling in November, 1821.*

In this case there was no direct certainty of infection from glanders; but it was extremely probable, as the man had been attending and grooming some glandered horses, and stated that he had frequently washed the ulcerated nostrils of these animals. In the same month, the case of a groom, named Kliech, occurred, and was communicated to the medical council of Silesia by Dr. Weiss, the district surgeon of Neumarkt.† In 1822, an interesting description was given by Tarozzi of a pestilential disease which appeared, about the beginning of 1815, among several men in a stable at Ostiano, where there were glandered horses, and at a time when there was no trace of any such disease in the vicinity.‡ To these may be added the experience of Schrader,|| Travers,§ Numann,¶ Elliotson,** and Williams;†† Brera's treatise on Carbuncular Typhus, produced by Glanders;‡‡ the series of accurately detailed cases given by Profes-

* Rust's Magazin, B. xi. S. 480.

† Rust's Magazin, B. xi. s. 480. Hufeland's Journal for March, 1822.

‡ Omodei's Annals for 1822. || Hamburg Magazin for Jan. and Feb. 1823.

§ Inquiry into Constitutional Irritation.

¶ Vee-artsenykund Magazin. Deel ii. St. 2.

** Medico-Chirurg. Trans. vol. xvi. p. 171.

†† Medical and Surgical Journal, No. LVII. p. 156.

‡‡ Anthologia Medica for Sep. and Oct. 1834.

sor Hertwig* and my colleague Worlff;† the inaugural dissertations published on this subject at Berlin by Grub,‡ Krieg,|| and Barth;§ as also two remarkable cases communicated by Professor Alexander, director of the Hospital of Instruction at Utrecht, and accompanied by an accurate detail of the post mortem phenomena. I may also state, that my friend Berndt has devoted a particular section to this subject in his admirable work on pathology and therapeutics.

After adducing so many instances, drawn from the stores of modern observation, and confirmed by practitioners of various countries, the attempt to render conviction stronger may appear superfluous. But when, even in 1832, we find Parent Du Chatelet, in a report made to the Academy at Paris, maintain, that “none of the supposed infectious diseases of animals exercised an unfavourable influence on the health of man;” when further, in a widely perused journal published in this city,¶ we find a practical physician, Dr. Krüger Hansen, in examining Wolff’s proposition above mentioned, denying the existence of infection from glanders in general, and its pernicious effect on man in particular; and in pretty plain terms attributing all the bad consequences which arise from such infection to inaccurate diagnosis and bad practice; finally, when we perpend the truth and importance of Goethe’s words, “that the truth must be constantly repeated, because error is preached upon every side, not only by individuals, but also by the mass of mankind,” it becomes our duty not to hold back any communication, however trifling, calculated to banish a pernicious con-

* Medicinische Zeitung Jahrg. 3, Nos. 46 and 47.

† Med. Zeitung Jahrg. 4, Nos. 1 and 2.

‡ Diss. sistens casum singularem morbi contagio mallei humidi in hominem translato orti, 1829.

|| De Typho Malioides, 1829.

§ De nonnullis epidemiis et epizootiis simul regnantibus, earumque mutua male contagiosa, 1835.

¶ Grole and Walther’s Journal, Band 23. Heft 1. s. 58.

fidence, and to contribute to the knowledge and treatment of an animal poison, which though seldom observed, still when it once occurs, whether openly or in a latent form, threatens the most alarming consequences to human life. These considerations have induced me to publish the following seven cases of disease, consequent on actual or supposed infection from glanders. These have been derived partly from my own experience, and partly from that of my colleagues.

CASE. I.—The case observed by myself, the true nature of which I only began to suspect towards the end of the disease, and which I had afterwards an opportunity of verifying, occurred in 1830. On the afternoon of the tenth of July, lance-corporal J. Rudorff, of the Dragoon Guards, aged 25, five and a half years in the service, and by profession a saddler, was admitted into the regimental hospital. About a month previously, that is from the 20th of May to the 5th of June, he had laboured under ague, but had never been ill before during the whole period of his service. Some days before his last admission he had complained of agonizing pains in the upper and lower extremities, heaviness of the limbs, and sense of weight in the head, which subsequently changed to a most intolerable headach, so as to force tears from his eyes. When admitted, he complained chiefly of torturing pains in the limbs, and weight of head. His face was red; skin hot and dry; his thirst great; pulse hard, full, and frequent. The disease was looked upon as inflammatory fever, (in the book I find it designated, at first, as acute rheumatism,) and he was ordered to be bled, and to have a cooling mixture, first of glauher salts, and afterwards of nitre, with lemonade for drink. The blood exhibited a firm coat of buff, and the violence of the headach, as well as the hardness of the pulse, diminished remarkably after his bowels had been freely opened; but the pains in the limbs, and the frequency of pulse, continued. On the 12th, tumours like boils formed on the outside of the left knee and on the right tibia; these gradually increased in size, passed deep among the muscles, and seemed to

contain a fluid. They presented no inflammatory appearance externally, or at most a pale blush, but they were excessively painful on motion, and communicated a burning sensation to the touch. On the 13th, the congestion of the head again returned ; the patient was sleepless, and occasionally delirious ; the pulse 100 in a minute ; the skin dry and burning ; the face red ; thirst urgent ; tongue dry and loaded. Eight ounces of blood were again drawn ; and the patient took calomel, at first three grains, and afterwards a grain every two hours. The relief, however, was but of short duration. From the 14th to the 15th the fever increased ; the head became remarkably affected ; there was strong pulsation of the carotids ; delirium more constant ; and during its brief intermissions, the patient complained chiefly of violent pains in the legs and head. Twelve leeches were applied to the temples, mercurial frictions were made in the vicinity of the tumours, and the knee was enveloped in tow, but without any benefit ; the fever increased, assumed more of the nervous type ; the manifestations of consciousness became fewer, and tumours of the same kind began to appear on the thighs ; the skin became dry, rough, and hot, particularly in the neighbourhood of the tumours ; the tongue parched, chapped, and covered posteriorly with a dirty brown fur. The foregoing plan of treatment was abandoned, and an attempt was made to act on the skin by carbonate of ammonia, infusion of elder flowers, and baths ; and the painful swellings were bathed with a decoction of chamomile flowers, containing acetate of lead, and tincture of opium. The patient perspired copiously, and had some sleep after the bath ; but there was no improvement in his symptoms. Tumours began to appear on the left knee, the right thigh, the left forearm, the neck, and the left upper eyelid, partly of the foregoing description, partly livid, round, some filled with pus, others hard to the touch, of various sizes, and all extremely tender. The day before the patient died, I stated, on visiting him, that his whole disease had been of a peculiar character, and that he must have laboured

under the influence of some specific poison. Whereupon, a patient of the same squadron, who had been admitted during his illness, observed, when questioned as to the patient's mode of life latterly, that he had been occupied for a long time in the sick horse stable. The matter was now cleared up at once; and I immediately recollected the effects of the poison of glanders of which I had read much, but unfortunately, as generally happens to those who have not seen them, had not thought of before. The patient's condition in the mean time became still more putrid; his pulse small, frequent, 125 in a minute; his face rather pale than red; his skin dry and hot: he remained almost constantly in a state of unconsciousness and low delirium, and passed involuntarily quantities of abominable fæces, smelling like carrion. A warm alkaline bath, and cold effusion, roused him for a moment; he then sank with all the appearances of profound collapse, paralytic dysphagia, sopor, cold perspirations, and finally spasmodic contractions of the muscles of the face. He died on the 18th of July, at nine o'clock in the evening.

On opening the body, the pale red boils and the livid pustules were found considerably collapsed; the latter were partly filled with yellow thick pus, the former with an offensive fluid of a chocolate colour, which was also infiltrated among the muscles, and had made its way down even to the bones at certain points. The muscles were in general sound, but some spots appeared pale and softened. The venous blood had a remarkably watery appearance, was discoloured, and appeared as if mixed with a slimy fluid, as far as the right auricle; the crural vein of the right side was very much distended, and its blood seemed to be mixed with a mucous or puriform fluid; the internal coat of the vein was of an ash gray colour at various spots, and covered with a puriform exudation. This part of the dissection appeared to me peculiarly interesting, on bringing to mind the description of the state of the veins in Czarna Krosta, (*pustula maligna*), which I had read in the Dorpat Clinical An-

nals. There was no trace of inflammation in the cavities of the brain; the superficial veins contained a larger quantity than usual of dark fluid blood; a spot on the centre of the posterior lobe presented some superficial ramollissement; on the right side there was a superficial abscess about the size of a two groschen piece, and about one-eighth of an inch in depth, containing matter similar to that found in the pustules. The lungs were normal; the pericardium contained about two ounces of dark brown fluid; the liver was softer than natural, the bile of a clear yellow colour.

After the fatal termination of this case, my first inquiry was directed to the disease of the horses which the patient had attended. At that time, however, I could not obtain any accurate information. It was stated that some of the horses had glandular affections, and disease resembling glanders. I afterwards inquired from the serjeant of the troop, and learned from him, that both the horses which Rudorff had groomed were killed in consequence of being glandered; one on the 1st, and the other on the 25th of October, 1830. How he had received the infection, it is difficult to say; but a dragoon stated to me, that he was in the habit of keeping his bread in the manger. It is also probable, that Rudorff, who was a saddler by trade, might have had a wound on his hand or some other part of his body.

CASE II.—The following case was observed and published in the official medical report for the province of Brandenburg in 1834, by my colleague, Dr. Grossheim, of this city. A military pupil of the veterinary school, named Lork, although not specially engaged in the care of diseased horses, had been in frequent contact with glandered horses while making examinations. Whether he had a slight injury on one of his fingers, or not, is not certain; at any rate, it must have been very slight, for he expressed himself doubtfully on the point, and merely said that such might have existed. The first inconvenience which he felt, consisted in a painful sensation in the fingers, without any evident tumefaction, but accompanied by fever, and

extending soon after to other parts of the body, particularly the joints of the upper and lower extremities, so as to simulate flying rheumatic pains. He was admitted into the general military hospital on the 3rd of February, and when examined, presented slight tumefactions over the right ankle and knee joint, both elbows, and some of the joints of the fingers ; these, however, were so slight, that they only appeared evident when compared with the corresponding parts on the opposite side. The accompanying fever was of an inflammatory character ; the pulse was full and rapid, the thirst extreme, his eyes shining, his respiration oppressed and difficult, great prostration, loss of appetite, nausea, loaded tongue, and constipation. With the view of moderating his fever, he was bled, and took glauber salts followed by an enema ; he then took infusion of elder flowers with antimonial wine and spirit of mindererus, with the view of acting on the skin. He perspired copiously, but without any diminution of the fever ; and the agonizing pains became more generally diffused, so that scarcely any part of his body escaped. To relieve the gastric symptoms, he took the compound infusion of senna and an emetic, but without any benefit.

From the 12th of February, several circumscribed doughy swellings, without any superficial redness, began to form deeply under the cutis, on the arms, legs, and head. During this period his breathing was very much oppressed, so that it was thought necessary to bleed him : he afterwards took calomel in combination with camphor and opium, and vapour baths, which had the effect of producing a temporary alleviation of his pains. He was then attacked with diarrhœa, followed by remarkable prostration ; and his disease began to assume the character of nervous fever, attended with great congestion of the brain. He had delirium, alternating with consciousness, heat of scalp, suffusion of the eyes, dry tongue, small and rapid pulse. Some of the furuncular tumours were extremely painful, particularly one about the size of half a hazel nut, situated over the left parietal

bone. On the surface of the latter several minute openings formed, from which a brownish puriform fluid exuded. A number of miliary vesicles made their appearance on his face.

The treatment, which was regulated by the symptoms, consisted in giving mineral acids, Dover's powder, nitre with camphor, calomel and digitalis, and the application of leeches to the forehead and temples, with cold affusions. The patient died on the night of the 4th of March, with the usual symptoms of typhus in the last stage, as sopor, muttering delirium, sinking of the countenance, involuntary discharge of urine, &c. The evening before his death he had copious general perspiration.

On dissection, the vessels of the dura and pia mater were found greatly congested, as also the substance of the brain, and the choroid plexus : there was a sero-sanguinolent effusion in the cavities of the brain and spinal cord. A similar effusion was discovered in the pericardium and both pleural cavities ; and on the left pleura costalis there was a quantity of extravasated lymph. On cutting into the liver, a large quantity of bloody serum gushed forth ; the vessels of the stomach and upper part of the small intestine were strongly injected, and the larger arterial branches presented at certain spots a remarkably red colour. Oblong suppurating tumours, about the size of half a walnut, and containing a more or less brownish fluid, of tolerable consistence, were found in many parts of the muscular substance of the upper and lower extremities, viz. in the belly of the extensor digitorum communis, brachialis internus, supinator longus, and gracilis, of the right side ; also in the brachialis internus, and peroneus longus of the left side : these tumours were covered by the aponeurotic extensions. The neighbouring, and in particular the subjacent muscles, were wasted ; but there was no trace of inflammation or vascular congestion.

CASE III.—I am indebted to Physician General Dr. Lohmeyer, for the following case, extracted from the Army Medical Reports. A soldier of the 1st Hulan Regiment, aged 23, complained on the 2nd of July, 1834, of pain in the right index

finger, arising from a slight injury of the skin, the cause of which the patient could not exactly state. Although the injury seemed trifling and inconsiderable, the whole hand was inflamed and swollen, and the swelling and inflammation became more extensive under the use of warm, soothing applications ; painful inflammatory boils appeared on the hand, forearm, and elbow joint, and the axillary glands became swelled and tender. He then got feverish, and was admitted into the hospital on the 6th July, where the warm applications were continued, and frictions with mercurial ointment employed. Up to the 12th the swelling of the axillary glands diminished, but the boils on the forearm and hand suppurated, and were opened. At the same time the injured spot on the index finger increased in size, its edges became tumid, its surface foul, and it discharged a fetid, greenish yellow pus. It was powdered with the red oxyde of mercury, and covered with simple dressing. It was now ascertained, that the patient had for several weeks been attending two horses labouring under glanders, and which had been subsequently killed on that account. Although the feverish symptoms had almost entirely disappeared, and the patient did not feel ill, inflammations and swellings appeared from time to time on various parts of his body. He took acids, decoction of bark with muriatic acid, and the ulcers were dressed with turpentine and tincture of myrrh ; under this treatment the boils improved. On the 26th of July, a painful erysipelatous inflammation appeared on the left ankle, and two days afterwards another of the same kind on the right wrist. Feverish symptoms set in, and the patient, who had hitherto been walking about the ward, felt so weak as not to be able to leave his bed. Dry applications, and subsequently, when the pain and inflammation increased, leeches were employed, but without relief ; warm poultices and decoction of bark reduced the swelling ; but instead of the pain felt before, he had now a numbness in the affected parts, with loss of power in the limbs, and increase of fever and emaciation. Up to the

12th of August, the fever had assumed a low type, which became gradually more developed; new swellings of the foregoing description, and with a doughy feel, formed, and when they disappeared, were replaced by fresh ones on other parts of the body; none of these, however, exhibited marks of fluctuation.

Infusion of valerian and sulphuric acid were added to his drink, but the fever and weakness increased, delirium set in, and a number of small boils, raised on a hard inflammatory base, formed on his face, neck, and breast; these were about the size of a silver groschen, and presented on their surface purulent vesicles, which broke and discharged a fetid, bloody sanies. The delirium and oppression increased up to the 17th of August, when the patient died, having remained six weeks and three days in hospital.

CASE IV.—The following case, witnessed by Surgeon Kratzenstein of Stralsund, and attended with fatal effusion of pus into several joints, is taken from the Reports of the Board of Health for the Province of Pomerania, published in 1835. A labourer, aged 46, of delicate constitution, and who had suffered much from illness during his childhood, but had subsequently enjoyed good health, with the exception of an attack of scabies about ten or twelve years previously, received a trifling injury of the little finger of the right hand from a nail, while gathering up some provender which had remained in the manger after a strange horse. He took no notice of the injury, and continued to discharge his usual offices until the fourth day, when he felt so unwell as to be obliged to remain at home. On the eighth day after the receipt of the injury he sought for medical advice. He had at this time violent fever, with severe gastric symptoms, the right hand was greatly swelled and there was an erysipelatous blush over the little finger, and the wrist joint. At both these spots, a slight fluctuation indicated the formation of matter. He took carbonate of ammonia, an emetic, and aperients; and the parts were sprinkled with flour

but without any benefit. As soon as fluctuation shewed that pus was formed, the abscesses were opened by a longitudinal incision, and a considerable quantity of healthy pus discharged. A few days afterwards red, painful spots, about the size of a dollar, and slightly swollen, appeared on the wrist and elbow joints of the left arm; the latter was opened by a free incision, as it exhibited proofs of the formation of matter, and a quantity of pus discharged. Both feet were swollen, but without any redness or marks of suppuration. The fever continued with decrease of strength, delirium, and copious suppuration, the matter becoming of a more unfavourable character. Bark, acids, wine, broths; the local application of warm aromatic dressings, and enlargement of the openings into the abscesses, proved ineffectual. The right wrist joint was first attacked with suppuration; in spite of bandages and splints, spontaneous dislocation took place, the ends of the bones were denuded of periosteum, so that at each movement a grating was felt in the metacarpal bones and phalanges. Suppuration also took place in the right ankle, and notwithstanding the same applications were made, the ligaments became loose, and all the bones of the foot seemed capable of free motion. The man died after eight weeks spent in great agony. Dr. Rhades, the compiler of the Provincial Report alluded to, seems to think it was very probable that this peculiar malignant disease arose from the contagion of glanders, although he had not been able to find out anything concerning the horses which had been in the stable where the patient was supposed to have received the infection.

CASE V.—The following interesting communication appears among the reports of the Board of Health for the district of Marienwerder, as detailed by District Surgeon Genzmer:—"A groom named Knoke, aged 27, became ill about the beginning of the month of November, 1835. He had been attending glandered horses for two months previous to this period. He complained at first of dislike of business, languor, loathing of

food amounting to nausea, and constant tendency to vomit. I ordered him an emetic, which was followed by a copious discharge of mucus and bile with great relief of his symptoms. The following days he complained of pain in his chest, and in all his limbs, but particularly of an affection of the head. At night he became quite feverish, but towards morning had copious perspirations, with remission of his symptoms. He was ordered to take carbonate of ammonia, and nitrate of potash. On the fifth day, he complained of pains in the right arm, and in the right and left forearm, as also in the right leg, immediately over the ankle. On examining these parts, I found at the painful spots cold tumours, partly of an oblong, partly of a roundish form, having a doughy feel, but without any alteration in the colour of the skin; the fluctuation of fluid could be felt in them. They were rubbed with mercurial ointment and camphor. These measures having effected no change, the boils were all dressed with the emplastrum fœtidum, and an infusion of arnica with carbonate of ammonia was given internally. The fever now became exacerbated at night, and the morning perspirations were extremely profuse, attended with a remission of the symptoms. A higher degree of vital activity began now to manifest itself in the tumours; they inflamed and burst after using the remedies above mentioned for the space of four days, giving exit to a darkened sanies; they were then dressed with stimulating applications. The patient now began to improve, and was ordered to take light tonics, but the ulcers still had an indolent appearance, and formed deep suppurating cavities. An ointment composed of iodine, and unguentum elemi, was ordered for dressing, and the patient took iodine made into pills with extract of acorus calamus. This was followed by an improvement in the condition of the ulcers, they assumed a more favourable appearance, began to discharge a more healthy pus, and at present (March, 1836) they are nearly well. The patient is improved in strength, and it is

expected that he will be quite restored to health in two months at farthest."

CASE VI.—" John Kaletzki, a groom, aged 34, was appointed to fill the place of Knobe, when the latter became unwell, a task which he performed much against his inclination. After having discharged this duty for about six weeks, he was attacked on the 6th of January, 1836, in the same manner as his predecessor, except that the pneumonic symptoms were more violent, and threatened suffocation. He was bled with relief, and the blood exhibited a slight coat of buff. On the third day the pulmonary symptoms returned, leeches and a blister were applied, and relief again obtained; internally the patient took a cooling mixture of nitrate of potash. Four days afterwards, cold but painful swellings appeared on the clavicles, and subsequently on the left arm, and on both thighs. The tumours had the same appearance as those observed in Knobe, but Koletzski complained of more pain, and his fever was much greater, and had more of the asthenic character, so that the nitrate of potash was changed for carbonate of ammonia. The patient then complained of pain in the throat and difficulty of swallowing, but there was no appearance of injury in the interior parts of the mouth. On the 18th, Dr. Burkhardt saw the patient with me, and recommended a solution of oxymuriate of mercury in full doses, which was administered for some days. The fever, however, increased daily, delirium prevailed, particularly at night, and the patient had copious alvine evacuations of a putrid, offensive character, attended with evident sinking of the powers of life. He now took infusion of valerian with muriatic acid, and subsequently infusion of arnica; mercurial ointment with iodine was rubbed over the tumours, but without alteration in their appearance or character. The difficulty of swallowing increased, so that the patient was unable to swallow a spoonful of medicine; and as there was no apparent alteration, it was inferred that this arose from paralysis of the œsophagus.

The patient died with all the symptoms of general collapse and exhaustion, on the 5th of February."

CASE VII.—Dr. Grimm of Kempen, details the following case as observed by him, and published in the official Medical Report for the District of Posen. A healthy man, aged 23, residing at Kempen, who had the care of a glandered horse, after a slight superficial injury of the right index finger, got a violent inflammatory swelling, which soon extended over the whole hand, became extremely painful, and in a few days increased so much in extent as to comprehend the whole forearm as far as the elbow. Some glands about the elbow, and afterwards those of the axilla, became swollen and painful, accompanied by feverish excitement of the pulse. Dr. Grimm ordered soft poultices and frictions with mercurial ointment; the wound on the finger, which presented a foul appearance, with tumid edges, and greenish, foetid discharge, was dressed with red precipitate. Constipation was removed by the use of calomel, and the patient took muriatic acid with decoction of bark, and acidulated drinks. The swelling of the axillary glands diminished under this treatment; but boils of different sizes, and of a purplish hue, formed on the arms and various other parts of the body, presented marks of fluctuation, and when opened discharged a thin whitish pus. The fever increased, the patient lost strength, and the limbs became useless from the number of tumours preventing motion, so that he was obliged to remain in the horizontal posture; one in particular appeared on the left ankle joint, attended with severe pain. Eight leeches were applied without benefit, but he was somewhat relieved by warm poultices. He also took bark, but without any improvement in his symptoms. The tumours increased in number, but no longer presented marks of fluctuation; he suffered greatly from constriction of the chest, with delirium, and all the symptoms of typhoid fever. A number of small boils, about the size of a split pea, with hard, inflamed bases, appeared on his face, neck, and breast;

on the centre of these, transparent vesicles formed, which burst, and discharged a foetid brown sanies. The patient died on the fortieth day of his illness. No autopsy was allowed.

The foregoing cases, analogous in their results, and admitting in general of being placed in the same class, will serve at least to add to the repeated proofs, that glanders is undoubtedly a poison, and one of a very pernicious kind to man. That its effects on man are different from those observed in the horse and ass, is by no means surprizing ; the same thing occurs in the case of mortification of the spleen and hydrophobia. Is not the peculiar symptom which has given its name to man, wanting in the dog ? Again, it is not so very strange, that the phenomena of infection from glanders, should be different in different individuals of the human species, even though we should meet with a greater variety of shades than is recognized by experience. Is it not the case with other diseases of contagious origin ? Do not syphilis and many other diseases appear under a variety of forms ? and are not the forms of glanders and button farcy different, according to the difference in their mode of origin, and of the tissue in which they form ? If the symptoms detailed by the foregoing authors differ a little in some points, is not the mark of acute rheumatism with which the disease generally commences in man, the fever at first seemingly sthenic, but attended with great frequency of pulse and weakness, and passing rapidly into asthenic nervous fever ; are not the formation of boils and abscesses, which correspond with the button farcy of the horse, as well as the whole *post mortem* phenomena highly characteristic ? Finally, that in one case, no infection arises from intimate communication with diseased animals, while in others the most fatal consequences arise from slight intercourse ; that the disease assumes one time the form of acute or chronic glanders, and at another time more of button farcy ; that occasionally it exhibits some resemblance to malignant pustule ; that in some cases it is rapid, in others creeping ; that it sometimes terminates in recovery, and at other

times in death. All these are circumstances which may depend, 1st, on the difference of form of the glanders or button farcy, which may be most dangerous to mankind in its acute form; 2ndly, on the peculiar stage of this form, and the difference in the intensity of the contagion connected therewith; 3rdly, on the difference of the surface on which the contagion is received, whether it be a wound or ulcer, or the mucous membrane of the nostrils, the conjunctiva, or a part of the skin covered with delicate epidermis, as the lips; or whether the infection has been communicated by the breath of glandered horses, and has entered the system through the lungs; 4thly, as in all contagious diseases, we must take into account the susceptibility of the individual, which may have been increased by some previous disease. We find, in small-pox, that after exposure to the same contagion, one person will have variola, another varioloid, and a third varicella, according to the different individual susceptibility of each.

Weighing those various circumstances, I cannot agree with the opinion expressed by some individuals, that the case of Rudorff was not one of glanders, but was produced by some other animal poison. On the contrary, I am inclined to think the severe form of disease observed in him, arose from the reception of glandered matter of a very bad character, probably on the mucous membrane of the nostrils, and while a great susceptibility to the poison existed. I am the more inclined to adopt this opinion, as the disease in this man exhibited many traits identical, not only with glanders in the horse, but also with distinct cases of the disease in man. In the character of the symptoms noticed in this case, there is a certain gradation easily recognized, from the purely local form, (as observed by Schrader, in a veterinary surgeon who scratched his finger with the nasal bone of a glandered horse,) to the form attended with fever and the formation of tumours and boils, and to the exanthematous affections terminating in gangrene, sloughing of the face, ulceration and discharge from the nose, &c. &c.

Finally, with respect to the treatment of glanders in the human subject, I must admit that, after a careful comparison of the results of all the cases which I have read, I have not been able to hit on any plan which I could pursue with any confidence in the management of future cases. It is undoubtedly of great importance, in the treatment of this as of other animal poisons, to endeavour by proper local treatment to fix and neutralize the poison before it gets into the system, and even when the commencing pains of the limbs, the incipient erysipelatous inflammation, the fever, &c. show that it has gone farther. Should this fail, and the disease become established, experience has shewn, that the assistance of art is at most extremely problematical; and that the cures which have taken place after the internal and external exhibition of mercury, the employment of turpentine, evacuants of all kinds, whether emetic, purgative, or diaphoretic, the use of tonic and nervous medicines, bark with acids, chlorine, excision or cauterization of the boils and pustules, injection of kreosote into the nostrils, &c. were in all probability the result of chance, or in other words, of the sudden and favourable exertion of the powers of nature, which, at other times, even under similar circumstances, does not manifest itself. Probably, in this matter also, the future brings us nearer to the goal. All we can do at present is to anticipate the evil, to attend carefully to all official medical reports bearing on the subject of glanders, and to remove as far as possible, particularly from the minds of medical men, the opinion still too prevalent, that this disease is not infectious, at least with respect to man. I trust the observations made in the foregoing communication, may elicit some further information.

ART. VIII.—*Report of St. John's Fever and Lock Hospitals, Limerick.* By WILLIAM J. GEARY, M. D., Physician to the City Infirmary, and Magdalen Asylum, and Assistant Doctor to the Hospitals.

(Continued from Vol. XI. p. 390.)

FROM the many instances in which we have noted the propagation of fever, either by direct contact with the sick, through the medium of wearing apparel, or from incautiously occupying apartments where persons had been recently ill of the disease ; we have been surprised how its contagious character could have been doubted, by those who had any fair opportunity of examining the subject. It is true that the circumstances attending epidemic disease are so sufficient in explaining the appearance of fever, that a considerable amount of evidence becomes necessary to assign its more general diffusion to contagion alone. One of the principal difficulties is to be found in the undefinable period, during which the disease remains latent, after the febrile poison has actually entered the system ; varying from a few days to as many weeks, a fact observable in variola and other eruptive effections, confessedly contagious.

Like other narcotic poisons, that of fever seems to operate in proportion to its concentration ; where it is at the maximum of intensity, its effects on the cerebro-spinal system are instantaneous, definite, and immediately manifested, while on the other hand, the amount being small, its effects are more gradually developed, most probably through the agency of absorption. The interesting cases furnished by Dr. Marsh are referrible to the former class, and from what we have observed, those cases run the heaviest and quickest course, which are speedily developed after exposure to contagion. This may be explained by the great shock the system sustains, by the absolute intensity of the miasm, by the pre-existence of the disposing cause in a high degree, or perhaps by all. For instance, during my attendance

on the Dispensary in March, 1827, after walking a good deal during the day, the weather being unusually warm, I visited a child, residing in a miserable, ill-ventilated cabin in Clare-street, who was represented to have been ill for several days before. The room in which she lay was very confined, and having no window, I had her brought to the door of the outer apartment, when she was found to be attacked with maculated typhus.

Immediately after examining the case, I was seized with nausea, vertigo, and a general chill. On returning home, I was unable to take any dinner, and on returning to my room from the table, I stated the circumstances, and my belief that I had caught fever. The disease set in that night, and so urgently did the acute symptoms supervene, that the head was leeches and blistered the next morning. The attack lasted for twenty-one days; and among other distressing symptoms *singultus* continued for seventy-two hours.

It will be inferred, that my frequent intercourse with the sick, had established a predisposition so strong, that it only required a small amount of exciting cause to produce the disease. Of this predisposition (if it existed) I had no appreciable knowledge, as I never felt better than on going into the house. The illness appeared to me, then, to have proceeded from the state of concentration which the miasm had acquired, by the absence of cleanliness and ventilation. Other causes than the specific emanations from the bodies of the sick, are capable of producing the disease, and with as rapid intensity. Among the most remarkable that came under our notice, were those arising from the continued operation of the effluvia of putrid animal matter: we have seen a very decisive instance of this kind, arising from the effluvia of a foul and ill-conditioned wound of the head, succeeding an extensive fracture of the right parietal bone, with laceration of the dura mater. It occurred in 1819, to my father, while attending an inquest on a young man, who died from the injury. Having sat in the draft, he perceived the "*scent from*

the wound” intolerably offensive, and felt that he had then inhaled the poisonous dose. In the course of the day he became more sensible of his situation; on the next evening he was obliged to yield, and thenceforward continued in severe and dangerous fever for many days. A very singular case of fever occurred here, being the last year; the exciting cause was the morbid impression of camphor on the olfactory nerves. Miss——, a young and healthy lady about twenty years of age, walking by a chemist’s shop in George’s-street, was suddenly overpowered by the odour of camphor as she passed the door. She turned to her sister and exclaimed, “Oh, did you smell the camphor—I am sure to have a fever now!” her predictions were too truly verified; she was attacked with the disease that night, and had a serious illness of several days’ duration. This case is the more remarkable, as one of the lady’s sisters had fever some years before, which was attributable to the “smell of camphor!”

We have endeavoured to trace the course of fever, through several members of families admitted into the hospital during the year. The inquiry furnishes 130 instances in which the disease was manifestly transmitted through members of different families; the total number of persons through whom the contagion was traced being 341, viz.

8 members of a family admitted in 1 instance.			
7	do.	do.	in 2
6	do.	do.	in 2
5	do.	do.	in 6
4	do.	do.	in 9
3	do.	do.	in 21
2	do.	do.	in 89

In each division the members of the family were admitted together or in close succession. Some had the disease from having come into close contact with the sick, others from sleeping in the bed covering in which they lay; and again, more from inhabiting the rooms recently tenanted by the infected.

We witnessed a well marked instance of the latter in the person of a Roman Catholic Clergyman who was seized with fever, on being translated to a parish, where the curate had but recently fallen a victim to the disease, (1835.) He occupied the room where his predecessor expired, and though every precaution had been adapted to fumigate and disinfect the apartment, he was attacked with a lengthened and malignant illness. This zealous and estimable class of society, are constant victims to fever, which seems attributable to their close contact with the sick, as well as the length of time they are obliged to remain in unwholesome and confined apartments; breathing a deleterious air, and exposed to the continued operation of contagion. This may be looked upon as the probable reason of the frequency of this disease among them; even in cases where a constitutional predisposition can be hardly said to exist. In observing the transmission of fever, we have been often struck by the dissimilarity of supervening cases, with those from which they originated. Peculiarity of habit exercises at all times a well marked influence on the character of disease, and the modifications which are sometimes created do all but cloak the specific affection in obscurity. We have noted the first form of fever in a family as unequivocally typhoid, and though there could be no doubt, that other cases which appeared among them had their source in this case, they each assumed characters very opposite, the typhoid miasm not appearing to excite typhus; and on the other hand, that form succeeding, where a different variety constituted the source. This is certainly at variance with the strict identity which distinguishes the *eruptive character* of variola, rubeola, scarlatina, and other contagious diseases; though we must admit, that there is often but too much cause to regret the variability of the accompanying fever, when these affections prove epidemic. In a family consisting of seven, we have seen one aged seven years seized with typhus, and in a few days subsequent, four others taken ill. Of these one had scarlatina, two simple fever, and the

fourth aged fifty-six years, had fever with senile pneumonia: and again, towards the close of 1828, measles having become epidemic in this city, we frequently noticed, in the months of November and December, several grown members of families attacked with fever, two or more days after the measles had appeared among the children. It is of consequence to state, that fever was less prevalent, during this and the two succeeding years, among us, than at any period from 1817 to the present.

We abstain from extending this paper beyond its legitimate extent, by adding further instances of the transmission of fever from person to person. Sufficient, however, has been adduced to prove the fact, both by the *immediately sensible* impression which the introduction of miasm into the system has excited, on coming within the due limits of its action, and the *more gradual* (though not less satisfactorily traced) diffusion of the disease through different members of families admitted into the hospital. In treating this subject we should not forget, that the *contagious property* of fever, like that of hooping-cough, and scarlet-fever, is only known by its effects thus differing from small-pock, the plague, and other diseases which are communicable by direct inoculation. In fine, when we have carefully connected the circumstances of contact of the healthy with the sick in person, through the medium of wearing apparel, by the occupation of apartments recently tenanted by the sick, we have accumulated the principal amount of evidence which such an inquiry admits of; and should an individual be seized with fever, after an exposure under these circumstances, we cannot see any difficulty in assigning the attack to contagion.

Children are much more liable to fever than is generally supposed, and to the little apprehensiveness of disease being transmitted by them, may be attributed the spread of disease through families in many instances. It will be seen underneath that nearly one-sixth of the admissions for 1836 were under ten years of age, a fact which bears out what we have stated, and is also a satisfactory proof of the increasing con-

fidence which public hospitals are acquiring from the community.

TABLE *showing the Number admitted at stated Ages of five Years ; their Relation and average Mortality per cent. from 6th January, 1836, to 5th January, 1837.*

Ages,	5	10	15	20	25	30	35	40	45	50	55	60	65	70	Over	Total.
Admitted,	81	489	762	701	362	304	100	203	70	82	23	36	2	10	2	3227
Died,	2	13	18	37	22	27	12	45	13	22	5	12	1	5	1	235
Average Mortality per cent.	2 $\frac{1}{4}$	2 $\frac{1}{2}$	2 $\frac{1}{4}$	5 $\frac{1}{4}$	6	8 $\frac{3}{4}$	12	22 $\frac{1}{4}$	18 $\frac{1}{2}$	27	21 $\frac{1}{2}$	33 $\frac{1}{4}$	50	50	50	7 $\frac{1}{4}$

By comparing the number of admissions and of deaths for successive terms of twenty years, it will be found, that the susceptibility to fever diminishes, while the proportionate fatality augments with the increase of age; viz. for the first term, in 2033 admissions, the deaths were 3 $\frac{1}{2}$ per cent., while for two succeeding periods of equal duration, the mortality reached 11 and 25 per cent. respectively, the admissions being 969 and 211. Of the entire treated for the year, full two-thirds were under twenty years of age; and only one-third of the mortality occurred within that term. These deductions show satisfactorily the influence which age has, both on the extent and fatality of fever cases. We have already stated that fever maintains a steady augmentation amongst us. The annexed return shows to what extent; and while we average this report, (July 29,) the numbers continue to pour into the hospitals to the same afflicting amount they did at this season last year. All the exciting and predisposing causes are present in an unusual degree; even constitutions the most habituated to its presence are obliged to yield; and we regret we cannot indulge in any fair prospect of an improved condition of the poor, from which alone we might hope for a check to this scourge. Provisions now are scarce, bad, and dear; ardent spirits abundant and cheap; the

weather variable and damp ; and there is a great want of employment among the operatives and labourers. In a word, we have seldom known such absolute want and distress as is felt in Limerick at present. The parliamentary session has closed, and with it all the expectations of relief which its opening held forth. The discussions on the various measures of charitable relief submitted to its consideration have proved angry and unsatisfactory ; and there appears but too much reason to apprehend a further delay to the final settlement of the Poor Law question ; a measure at once the most effective in checking the progress of contagious disease, and elevating the moral character of the lower orders.

QUARTERLY RETURN, *showing the Number admitted during each Season, from 6th January, 1836, to 5th January, 1837, the Mortality and general average per cent.*

	Admitted.			Died.			Average and General Mortality.
	Male.	Female	Total.	Male.	Female	Total.	
Spring,	259	315	574	18	24	42	7 $\frac{1}{3}$
Summer,	358	557	915	31	28	69	7 $\frac{1}{2}$
Autumn,	375	610	985	25	37	62	6 $\frac{1}{3}$
Winter,	340	413	753	26	36	62	8 $\frac{1}{2}$
Total,	1332	1895	3227	100	125	235	7 $\frac{1}{2}$

The return does not include the number which remained in hospital at the close of 1835, nor the inmates of the establishment, many of whom were attacked with fever during the year. Mr. Swayne, the resident apothecary, and his wife, the house-keeper's daughter, the assistant register, and several of the nurses were ill, many of the latter more than once. The increase of disease was most observable during these periods, when the causes to which we have already alluded were most urgent. The influence of sex appears in favour of the males, one-third less having been attacked than of the females. The greater delicacy of the latter, as well as the disproportionate amount of labour they undergo, slenderly clothed, and bare-

footed, seem to be the principal cause of their greater susceptibility to fever. As regards the mortality, there does not appear to be any remarkable difference, the proportion being 1 in $13\frac{1}{2}$ of females, and 1 in 14 of males; though that for six years, from 1814 to 1820, gave the proportion of females as 22 to 15, a relative average, which commonly exists. The amount of deaths is much greater during the winter than any other season, which is owing to the complication of chest affections among persons advanced in life, who are subject to the periodical aggravation of bronchial and pneumonic attacks at the time. Among the assignable causes of mortality, too much importance cannot be attached to the period of disease at which the sick are brought to hospital. It seems a well established point, that the chances of recovery are in the exact ratio of the period of illness at which the sick come under treatment. In this point of view, it is obvious that every thing is to be gained by the timely application of those remedies which are of known value in arresting or moderating symptoms, where they are resorted to before the disease has obtained a fixed development; we allude to emetics and purgatives principally. The beneficial result of early treatment is clearly represented in the following table, which shews the day of illness on which each case was admitted, number of cases, and deaths, with the ratio of mortality per cent., exclusive of forty-two Lock cases, fourteen infants sent out to nurse, and seven who died without being entered on the medical books, the latter being registered for coffins, and are here included in the mortality, from 6th January, 1836, to 5th January, 1837.

Day of illness when admitted,	2	3	4	5	6	7	8	9	10	11	12	13	14	Over	Total.
No. of Cases,	84	319	484	538	472	289	479	170	131	28	41	16	67	88	3206
Deaths,	3	10	22	28	33	30	36	15	21	2	5	1	5	24	235
Mortality per cent.	$3\frac{1}{2}$	$3\frac{1}{2}$	$4\frac{1}{2}$	$5\frac{1}{8}$	7	$10\frac{1}{2}$	$7\frac{1}{2}$	9	16	7	$12\frac{1}{2}$	$6\frac{1}{4}$	$7\frac{1}{2}$	$27\frac{1}{2}$	$7\frac{1}{4}$

This gives the mortality of all admitted from the second to the fifth day, as ranging from $3\frac{1}{4}$ to $5\frac{1}{2}$ per cent., while that from the sixth to the tenth day, varies from 7 to 16 per cent.; this is an increase of great magnitude, and could the public be sufficiently impressed with the fact, it holds out the most convincing inducement for the early removal of the sick to hospital, a regulation, it should be remembered, not of advantage to the sick alone, but one of deep moment to the family, as such a course would be withdrawing the most apparent source of fever from amongst them.

It is not necessary to remind those acquainted with extensive hospital practice, how the deaths are run up by circumstances entirely beyond the control of the physician, among these may be enumerated, the moribund state of many; of this class we had seven, in such an extreme state of disease and exhaustion, that there was not actually time to have them transferred to the medical books before they expired; and again of those admitted in the last stage of disease and suffering, twelve died on the first, and eleven on the second day after they came in.

Thus thirty should be deducted from the general mortality, which leaves a fair average of 1 in $15\frac{1}{2}$. Further, the advanced age and feebleness of others; thus of fifty admissions aged 60 years and upwards, nineteen died, being 1 in 3; and again, referring to the influence of protracted removal to hospital, on recovery of 541 treated from the ninth day, upwards of seventy-three died, being 1 in 7, while of 2595 taken in from the second to the eighth day of illness, 162 died, being only 1 in 23 nearly. To these we may add the complications of fever with organic diseases, as phthisis, dysentery both acute and chronic, diarrhœa, &c. some of which remain latent till brought into action by the constitutional excitement there present. Among these, we have found chronic dysentery the most troublesome, arising from the use of unwholesome and innutritious food, and exposure to damp and cold.

Dr. Mateer, in an interesting paper, to be found in the

Dublin Medical Journal for September, 1836, states, that he has not found the influence of trade or occupation, in the production of fever, so great in the manufacturing town of Belfast as may be supposed. Our own observations coincide with this view. We have a tabular view before us, which shews the number in families of each class of 2416 persons admitted from the city parishes, and the proportion they bear to each other; though the exact relation to the general population cannot be determined, as there is considerable difficulty in ascertaining the amount of each trade. However, as may be expected, the labouring class being the most numerous, constitute the largest number, averaging one-half of the entire; and including all we find that more than one-half of those treated for the year cannot be said to be of any trade, viz., females and children. We have elsewhere stated that the patients received from the county constituted one-sixth of the whole. The exact number is 567. They principally came from damp, boggy districts, and their abodes were distant from Limerick from five to sixteen miles.

To the interposing protection of the Fever Hospital in this city, we essentially attribute the escape of the humbler classes from the spread of pestilence and death; and to it there can be as little doubt, the wealthier part of the community owe their immunity from contagious fever. Dr. Currie supposes "every single removal into the house of recovery probably prevents, on an average, two or three cases of the disease," and Dr. Haygartt is of opinion, "that of twenty-three persons exposed to contagion, only one escapes."

We have now brought the statistical arrangement of the year's occurrences to a close, and though it proved a term of anxious exertion, we have the gratifying conviction that the united and zealous attention of all the individuals connected with the institution, have insured in a remarkable degree the success which attended the labours of the medical officers.

Number of patients admitted,	3,269
Aggregate number of days in hospital,	50,613
Average number of days of each patient,	15½
Total expenditure for the year, £1668 6 10	
Average expense of each patient, 0 10 2½	

BIBLIOGRAPHIC NOTICES.

A Treatise on the Malformation, Injuries, and Diseases of the Rectum and Anus. (Illustrated with Plates.) By GEORGE BUSHE, M.D., formerly Professor of Anatomy and Physiology, &c. New York. 8vo. pp. 299.

DR. BUSHE, once doubtless personally well known to many of our readers, received his medical education in this city, and in consequence of his distinguished abilities, having received a solicitation to accept the chair of anatomy in New York, left this country some years ago for that purpose. He finally settled as a practitioner in New York, and had just risen to the first practice, when, after long suffering, he sank a victim to consumption. For many years the diseases of the rectum had engaged his attention; and as he informs us—

“My opportunities for investigating them have been ample, and I may safely say, that I spared neither time, trouble, nor expense in endeavouring to arrive at just conclusions.”

The result is the work before us, to which he had just time to put the finishing hand when he expired. After a careful perusal, we cannot but regard it as a very valuable addition to the works already published on the subject. The style is concise, clear, and highly descriptive; and there is not an opinion of any importance but is supported by the author's actual observation and experience. As there is but one copy of the work (as far as we know) in Dublin, and as we are anxious to put our readers in possession of as much of the valuable matter contained in it as possible, we shall not take up more space in general remarks, but proceed to consider the subject more in detail; and by the copiousness of the extracts, it is our intention to render each person able to form his own opinion of the book. It begins with the anatomy of the rectum and anus, which offers nothing worthy of note, except that our author denies the existence of valves in the rectum; what have been usually described as such, he looks upon as merely mucous folds, which disappear on the distention of the intestine. To Dr. Houston he objects, that—

“ His method of investigation gave rise to his misapprehension of this piece of anatomy : one by filling the intestine with alcohol, and then opening it ; the other, by inflation and drying. In the first, the accidental folds are rendered permanent by the induration resulting from the action of the alcohol ; while in the second, the projections resembling valves are produced by the angles formed by the settling of the intestine during the process of desiccation.”

We confess we do not see the force of these objections, especially the last ; and according to his own shewing, (in a long note,) he has against him the respectable authority of Morgagni, Portal, Boyer, Glisson, and H. Cloquet. After all, how strange it appears, that a matter apparently so easy to demonstrate, should have given rise to such difference of opinion ; we cannot help thinking that it is a quibble about words, and that the mucous folds, which all allow to exist, do in reality act as valves in the moderately distended state of the rectum, but gradually disappear in proportion to the gradual distention of the gut, and finally become obliterated, and cease to act, when their further action would not only be useless, but serve to keep up an injurious retention of the *fæces*.

The treatment recommended for one form of malformation of the rectum and anus calls for some remark :—

“ Where the rectum is partially absent, an anus ought to be made if possible in the natural situation.”

To accomplish this, Dr. Bushe recommends, after dissecting to about the depth of two inches, or at most two inches and a half,—

“ If the bowel is discovered by its blackness and fluctuation, either a trochar, or what is better, a bistoury, should be forced into it, and the meconium evacuated. The opening thus formed should be maintained by tents of prepared sponge, &c.”

But he allows that most surgeons who have performed such operations have been unsuccessful. In short, so fatal is this operation, that we do not think Dr. Bushe would have recommended it, had he been aware of the admirable one proposed by Amussat. A translation of Amussat's paper is given in this Journal,* by the perusal of which such of our readers as are not acquainted with it would be well repaid. We may briefly state that Amussat was led to believe, from several fatal cases he had witnessed, and from experiments on animals, that death was to

* Dublin Journal, vol. ix. p. 338.

be attributed to the absorption of some parts of the fecal matter by the sides of the incision. He therefore obviated this by operating in the following way: having cut down to the bowel, he freed its connexion with the neighbouring parts in a great measure with his finger, he then drew the end of the bowel down with a hook beyond the lips of the wound, to the sides of which he fastened it with many points of suture. It was then opened, and the meconium let out; the artificial anus thus produced, kept open by spout tent, conical wax bougies, &c. The writer of this review can state, that a child thus operated on about a year ago is well and thriving, and the anus a remarkably efficient one.

The description of fissure of the anus is so very good, and the treatment of this distressing complaint, although offering little novelty, so very judicious, that we shall quote largely.

“ The disease so named is an ulcer, about the eighth of an inch in breadth, and from a quarter to an inch in length, situated immediately within the anus, generally on one or both sides, occasionally on the posterior, and still less frequently on the anterior part of the aperture. In the majority of cases it is confined to the mucous membrane, though occasionally it extends to the muscular tissue. Its inferior extremity generally corresponds to the edge of the external sphincter, though sometimes it is placed a little higher up or lower down. The base of this oblong ulcer is generally red, but sometimes grey, in consequence of the deposition of lymph. When recent, its edges are soft, pliant, and but little elevated; in proportion, however, as it becomes chronic, so are they more hard and prominent; changes which depend upon the interstitial deposition of adventitious matter from the irritated capillaries.

“ The surrounding mucous membrane is in its natural state in some cases, particularly when recent, but not unfrequently it wears an erysipelatous hue, and again assumes a livid aspect, and becomes soft. Women are more subject to this affection than men, which arises from their leading more sedentary lives, and consequently being more subject to constipation of the bowels. It generally occurs in the meridian of life; nevertheless I have treated a case in a girl of 18, and another in a woman 69 years of age.

“ In the majority of cases it is preceded by vascular tumours of the rectum, then it is situated between two of them, and is produced by the forcible passage of indurated fæces. In this act the vascular tumours are first prolapsed, and then separated, during which process the mucous membrane, rendered friable by inflammation, is ruptured. The contraction resulting from operations performed in this region, and the spasm of the sphincter, by opposing the free egress of the fæces, become a frequent source of fissure in the former, by disposing to rupture, and in the latter, by contusion and abrasion of the mucous membrane.

“ In three different instances I have mentioned, the laceration of the mucous membrane does not heal, because the primary affection still continues ; and even in some instances, as heretofore explained, the rupture is converted into an ulcer, though no primary affection existed.

“ Besides the causes now specified, inflammation, and consequent abrasion, may, from the columnar arrangement of the mucous membrane of the lower extremity of the bowel, give rise to one or more fissures.

“ In the commencement of this disease, the symptoms are not severe, being merely at one time a pricking or stinging sensation, at another, a slight smarting in a certain point of the anus, which, under the use of lavements, and low diet, subsides either altogether, or, after a few days, returns with some severity. The pain gradually increasing, becomes burning, sometimes lancinating, and when severe, throbbing. It is increased by forced expirations, as coughing, sneezing, and urinating. Every effort to discharge gas and fæces is attended with excruciating torment, which continues for one or more hours, attended with violent spasmodic action of the sphincter ani. So violent is the agony, that most persons thus afflicted put off the calls of nature, maintain the recumbent position, and some even avoid taking a proper quantity of nourishment, for fear of increasing the fecal mass. The pain is always increased by stimulating food ; and in females, during menstruation. Occasionally I have seen it assume a periodical character, which depended upon some peculiar state of the constitution. When the fæces are solid, they are slightly streaked with blood and matter ; and when more soft, are figured, and of small size. To examine the fissure, the buttocks should be forcibly divaricated, when its inferior extremity will be brought into view ; but in some rare instances we cannot accomplish this object, in consequence of its elevated site, and we are compelled to trust either to the introduction of the finger, or to the dilatation of the anus with the speculum for its detection. In a few cases, though it is immediately discovered upon separating the buttocks, we can only ascertain it by the means just mentioned.

“ The introduction of the finger is attended with great difficulty and torture, particularly when pressure is made on the fissure, which seems, in some instances, to be a mere depression, in others, to be surmounted by pretty high edges, while in a few rare instances, we only become cognizant of its situation by the increase of suffering in a certain point, under the same amount of pressure. When the pain is violent during and after stool, it is accompanied by fever ; and when it continues for any length of time, emaciations, hypochondriacis, and an irritable state, with a severe trace of nervous symptoms, ensue.

“ During the treatment of this disease, the patient should be kept on a low diet, and confined to the recumbent position. The common practice of administering cathartics, so as to produce fluid evacuations, cannot be too highly censured ; for such discharges sti-

mulate the ulcerated surface, and thus induce dreadful irritation and spasmodic contraction of the sphincter ani; therefore, the better plan is to administer daily an enema of flaxseed tea, and after its operation to cleanse the parts well. If the disease be mild, the application of the unguentum acetatis plumbi will be sufficient for its healing, and if there be much spasm of the sphincter, the extract of belladonna will prove a powerful auxiliary. Dupuytren recommended an ointment of this kind, the proportions being a drachm of the lead, and the same quantity of the belladonna, to six drachms of lard. Before I became acquainted with this practice, I was in the habit of applying the nitrate of silver to superficial fissures attended with spasm, and then introducing washes of lint, besmeared with a mass consisting of one part of the extract of belladonna, and seven of spermaceti ointment, a course of practice which has succeeded when Dupuytren's ointment has failed."

We may observe, that besides his own and Dupuytren's method of applying the belladonna and acetate of lead, we have seen another used by the Surgeon General with great benefit, namely, the continued application of a pledget of lint, wet with the following lotion: *Liquor Acet. Plumbi, ʒvi. Ext. Belladonnæ ʒii.*

"When a fissure will not heal under this treatment, and the patient continues to suffer, we should no longer delay the division of the sphincter, which never fails to give immediate relief, and to effect a rapid cure.

If the fissure is situated on the anterior or posterior part of the intestine, as the division of the sphincter and not the fissure is the desirable object, he prefers the incision to be made at either side; and thus—

"The inconvenience arising from the shortness of the space between the coccyx and verge of the anus, the proximity of the bulb of the urethra in the male, and the shortness of the perineum in the female, is avoided, as well as the difficulty of healing wounds made in the median line in this situation, in consequence of the friction created by the motion of the inferior extremities."

A dose of morphine immediately after the operation, low diet, and an emollient lavement should be exhibited daily, till the wound heals, which is in about three weeks. Both the chapters on neuralgia of the rectum, and spasmodic contraction of the sphincter ani, convey clear descriptions of these affections, and many original cases of much interest are given in support of his particular views. He offers no opinion as to the primary cause of the neuralgia, but regards it as of the same nature as nervous pains elsewhere. In two of his cases there was also tic

douloureux ; in the last, that of a delicate young lady, tic douloureux of the face, of a most aggravated character, immediately followed the cessation of the pain of the rectum. In another case, after antispasmodics, suppositories, enemata, the local application of extract of belladonna had failed, division of the sphincter completely succeeded.

Spasm of the sphincter he often found connected with irritation of the genito-urinary organs, irritable bladder for instance, with mucous deposit, and excess of lithic acid in the urine, and succeeded in removing it by the administration of soda, injection of the bladder, with solution of gum-arabic, hip-bath, mild laxatives, &c. The great omission here is the absence of any allusion (except in a solitary instance) to the use of the bougie. Now any one who has read Mr. Copland's excellent observations on painful spasmodic contraction of the anus, and his strong advocacy of the bougie for its removal must feel surprised at our author's silence on this head, as well as his not even mentioning Mr. Copland's name while treating of the subject, although other parts of the work shew him to have been quite familiar with that experienced surgeon's book. With Montagu our author regards the rectum to be frequently the seat of determination of blood, giving rise to discharges of blood of variable quantity and frequency, the formation of hæmorrhoidal tumours, inflammation, and mucous discharge. We shall confine our attention to " hæmorrhoidal tumours," or piles, because the chapter on them is not only the most valuable of the whole work, but that we really think it conveys to our mind a finer description of the disease than we have met with in any of the many books we have read on the subject; from the frequency of the affection, too, it possesses unusual interest.

" *Tumours.*—Generally after the blood has accumulated immoderately in the rectum several times, but occasionally as the result of the first congestion especially, when there has been no hæmorrhage, tumours form, in consequence of the opposition offered by the structure of the rectum to the escape of the blood. These tumours may be divided into two classes, the first is situated within, and the second immediately without the anus.

" Those situated immediately within the anus, vary in number, in many instances being so numerous, as to prevent the free discharge of fæces, while in other cases they are few, even solitary. Their size is as variable as their number, differing from that of a small pea to a pullet's egg. They are generally globular, and in many instances pedunculated, particularly when large, and subject to prolapse during defecation ; for under such circumstances they swell and suffer a constriction at their bases, from the contraction of the sphincter. Generally, they are of a dark red colour, and when pro-

lapsed, they become perfectly livid, in consequence of the obstruction created to the return of the venous blood; firstly, by the forced expirations necessary for the act of defecation; and secondly, by the constriction of the sphincter.

“I have repeatedly injected these tumours with coloured water, both from the arteries and veins, and when cut into while the fluid was projected, small jets were observed to issue from many points. I have frequently dissected them with the greatest care, and found that they were spongy, reddish, and contained both arteries and veins, the latter being most capacious, but always perfectly healthy. Their surface is villous, and generally bleeds when touched roughly, or scratched with the nail, the blood which issues being of a florid red colour. In many instances, I have been able to rub off exceedingly vascular and fragile adventitious membranes from their surface. Thus it would seem, that they may acquire an increase of magnitude in this way.”

Dr. Bushe has here given his opinion, that hæmorrhoidal tumours are made up of a sort of erectile tissue composed of arteries and veins, and in a long note he denies that a varicose state of the veins ever forms them. In this note he gives a list of the most eminent men who have written on the subject, from which it appears that the two latest, if not the best writers, Jobert and Dupuytren, consider them to be varices, while a whole host of others, among whom are Abernethy, Cruveilhier, and Champier, are of the opposite opinion, and agree with our author. But a later writer than any of these, and one whose opinion on any subject relating to pathology is entitled to the utmost consideration, but of whose work Dr. Bushe appears not to be aware, Dr. Carswell, considers that hæmorrhoidal tumours may be formed by either of these structures. From the view of an admirable plate of a rectum the seat of these tumours, the truth of the following opinion is manifest. “There are two forms of this disease, the more common of which depends on dilatation of the veins of the rectum; the other on a transformation of the dense cellular tissue of the margin of the anus into erectile tissue.”—*Fasciculus VI.*

“When these tumours are small, they are generally attended with slight heat and itching, but as they enlarge, they produce a disagreeable sense of fulness in the lower extremity of the rectum, and are prolapsed during defecation, after which they gradually shrink up, and by the action of the muscular apparatus of the anus, are returned to their original situation. In some cases, however, the sphincter becomes more or less relaxed, and these tumours, in descending, drag along with them a portion of the adjacent mucous membrane. Indeed so large is the protrusion, that persons thus afflicted are compelled to return it with their fingers; and many of them postpone the

calls of nature, until they are about to retire for the night, in consequence of the difficulty they experience, and the time they require to reduce it; and above all, as they can only effect this in the horizontal position. In many cases the protrusion occurs when the patient walks, or even attempts to ride in a carriage, and thus gives rise to great uneasiness and mucous discharge. Besides the protrusion of the mucous membrane now described, that of the pouch frequently takes place from the constant *nisus* these tumours are apt to create.

"In a few cases, when there is but one tumour, it is situated low down, and though not large, partially projects through the sphincter, and gives rise to very great annoyance. In a case of this kind, on which I operated a few days since, the mucous discharge was very considerable, the surrounding parts were much engaged, and the patient not only suffered from the friction which his clothes exercised on the tumour, but also from a constant teasing *nisus*, with occasionally irregular and painful contraction of the sphincter. However, the relief which he obtained from the removal of the tumour was immediate and permanent.

"In consequence of the irritation and pressure of the feculent matter, as well as the effort necessary to dislodge it, blood accumulates in the rectum during defecation, so that though there has been no previous hæmorrhage, these tumours may exhale blood; and in such cases it very often happens that it is squirted out from one or more dilated pores. Sometimes, though rarely, these tumours do not bleed for weeks or months; but I never yet have seen a case in which they did not bleed at some time. Generally, indeed, when they exist, they are the source of the sanguineous discharge which occurs, though previous to their appearance, the mucous membrane, as before pointed out, gave rise to it; and a most extraordinary fact, which I have verified repeatedly, is, that when they have been removed, the mucous membrane has again become the organ for throwing off the superabundant blood.

"When we consider the structure and situation of these tumours, we ought not to be surprized that they very often become inflamed, increase much in size, are attended with great pain, muco-purulent discharge, and disorder of the urinary organs. In this state, provided the tumefaction be great, the patient feels as if there were foreign matter in the rectum, straining ensues, and they are prolapsed; now the sphincter becomes affected spasmodically, and presses on their radices, giving rise to great suffering. The inflammation may subside in one, two, or three days, and then these tumours will either recede of themselves, or the patient be enabled to return them in the usual manner; but it sometimes happens, that the sphincter contracts with so much force as to strangulate them, and cause mortification; an event which generally effects a radical cure, though a few cases are recorded in which the issue therefrom was mortal. Of four cases of this kind which I have seen, three terminated favourably, one fatally.

"It not unfrequently happens, that in consequence of inflamma-

tion, in these tumours, small abscesses form in them, attended with a discharge of purulent matter from the anus, and more pain and irritation of this part than usual. Such cases are far from being uncommon, and are too often overlooked. To detect these small fistulæ, the finger ought to be cautiously introduced, and after a little exploration, a small depression, marking the fistulous orifice, may be discovered on each tumour thus affected. But should this attempt fail, the buttocks ought to be forcibly separated by an assistant, while the patient bears down; then with a strong light, and a probe of small size the sinus will be easily found. In the majority of cases but one tumour is fistulous, though I have occasionally seen two or more so.

“Occasionally these tumours are attacked with ulceration, and in such cases it generally seizes on many points at the same time, but seldom advances to any great extent. I have, however, seen a case in which three very large hæmorrhoidal tumours were one-half consumed. And in the twelfth chapter, p. 138, I have related another, which is interesting on account of the phagedenic character of the disease. Hæmorrhage is sometimes the result of the ulcerative process, as I had an opportunity of observing in the two following cases:—Mr. C., a gentleman advancing in life, of full habit, and subject to hæmorrhoids for many years, during a salivation which resulted from the mercurial treatment of a severe fever in the West, was attacked with more than usual uneasiness, and purulent discharge from the rectum while at stool. In a few days he began to bleed; and so much did this increase, that he repaired to New York, and became my patient. He was very low from loss of blood, and distressed in mind. I made a careful examination, and found four hæmorrhoidal tumours, one of which was as large as a peach stone, and ulcerated deeply. When he strained, all the humours were prolapsed, and florid blood issued freely from the ulcerated surface. I removed the tumours, and he soon regained his health. The other was that of a planter from Louisiana, who arrived here this summer, on his way to Paris to be operated on; but so low was he when he reached this city, that he felt unable to proceed, and, therefore, sent for me, and had the operation performed. In this case there were several ulcerated points on each tumour; and though they were superficial, the hæmorrhage from them was very brisk. Notwithstanding I have seen several cases of ulcerated hæmorrhoidal tumours, those are the only ones that were hæmorrhagic, and this I am inclined to attribute to the condensation which they generally undergo from repeated attacks of inflammation, previous to the commencement of the ulcerative process.

“Though these tumours maintain their spongy structure for years, yet it occasionally happens that, from constant irritation, they become transformed into a semi-cartilaginous mass, being firm, yellow, and nearly bloodless; I saw a case some weeks ago in a lady, who visited New York for the purpose of having them removed. I performed the operation, and two of the tumours are represented in plate, &c.

“In some cases, when we cannot detect any other lesion, and

these tumours appear perfectly free from engorgement, the patient who is generally of a nervous constitution, is affected with contraction of the sphincter ani, and exquisite pain, which, when violent, extends to the uterus, vagina, and external organs of generation in the female, to the perineum and testicles in the male, and to the bladder and urethra in both sexes. The constant tenesmus, stranguary, and dysury which it produces, wears the patient down, giving rise to sleeplessness, anxiety, and fever, and in some rare cases, so excruciating is the pain, that the patient must remain perfectly tranquil, as the least motion exasperates his sufferings to an intolerable degree. As these cases are not of common occurrence, the three following may prove interesting."

In three cases our author removed the tumours with complete success.

"The second class of tumours are those situated on the verge of the anus, though I have seen a few cases in which they extended a short way within this orifice, being in part covered with the mucous membrane. They are more or less livid, generally elastic, have an extensive base, and are formed of extravasated blood, which is encysted by condensed cellular tissue, and covered by a few fibres of the sphincter and fine skin of the verge of the anus. I have satisfied myself of these facts, by cutting off the prominent portion of the tumour, and then burning out the extravasated blood in the living body, and by cautious dissection in the dead. Sometimes the blood is absorbed, leaving no trace behind; occasionally, however, in consequence of the first, but more especially of repeated attacks, the superincumbent integuments and surrounding cellular tissue, become hypertrophied, and pendulous flaps or tumours, which in some instances, from the friction they are exposed to, obtain a rough or warty aspect, and become a source of great irritation. It not unfrequently happens, that when there is but one large tumour, it suppurates, and then gradually shrinks up. When small, these tumours are attended with itching, and sense of fulness and pain on pressure; but when large, the pain is constant, and is accompanied with more or less throbbing, and sometimes contraction of the sphincter. Indeed so severe is the anguish, that the patient is feverish and unable to walk, or take any other species of exercise, especially if supuration be about to occur.

"These tumours, when recent, are apt to be confounded with internal tumours, and partial prolapsus of the rectum. They may be distinguished from the internal tumours by being covered principally by the skin; by smoothness of surface; by our ability to glide the superincumbent parts over them; by their greater lividity; and finally, by their excessive hardness and freedom from hemorrhage. They differ from partial prolapsus of the rectum, not only by their density and lividity, but, by their great tenderness and tubercular form. However there is occasionally but one tumour, which is very

large and flat, with the mucous membrane stretched over its internal and inferior aspect, so as to simulate very much a descent of a portion of the mucous membrane. The diagnosis, however, is by no means so difficult, for, independent of its deeper colour, firmer consistence, partial cutaneous covering, and limited extent, if the finger be introduced, the tumour will be found to extend a considerable way within the sphincter.

“When these tumours undergo the changes I have before mentioned, they are liable to be confounded with excrescences which form about the anus, and are sometimes of a venereal character. They may, however, be easily distinguished from them by the history of the case, by the absence of other venereal symptoms, by the pliability of the skin; by the healthy condition of the surrounding parts; by the absence of a purulent secretion; by their lighter colour; generally, by their greater density and roughness; and finally, by being confined to the verge of the anus.”

The length of these extracts has somewhat exceeded our limits, but Dr. Bushe's delineation of the disease is so just and accurate, that we did not wish to spoil it by curtailment. In this account however, otherwise so perfect, we cannot but regret his total silence about a most important train of symptoms incident to long continued bleeding from piles, we mean those derangements in the circulation which proceed from reaction. Pain, dizziness, and lightness of the head, with noise in the ears, simulating the symptoms from a totally different cause, plethora, sometimes arise. Violent and constant palpitation of the heart with anasarca is another form. We remember to have heard the Surgeon General in his lectures relate a case of this description, where the original affection was entirely overlooked, and the treatment directed to the relief of the supposed disease of the heart, for a long time injuriously, but which was finally quite cured by the application of a ligature to the piles. Nine years after this operation we assisted in a similar one on the same gentleman for a return of the piles, he having enjoyed excellent health in the mean time. Two other cases have lately come under our observation, where the symptoms of the heart completely masked the primary disease. It would be a waste of words to insist on the importance of a right understanding on this head.

After discussing the treatment of congestion and hæmorrhage, depending on flux to the rectum, he proceeds to describe that of hæmorrhoidal tumours.

“When the tumours are exceedingly painful they should be anointed with the following salve, three or four times a day.

℞ Ext. Opii grana duodecim,
Ung. Cetacei unciam, M.

Sometimes the patient derives great relief from the application of cold water in a continued stream. Thus in England many of those afflicted with hæmorrhoids are in the habit of allowing the stream which issues from the water-closet, to strike against the part prolapsed, while defecating. When the sphincter ani is affected spasmodically, I have found the following ointment very useful :

℞ Ext. Belladonnæ drachmam,
Ung. Cetacei unciam. M.

Should the tumours be inflamed, leeches ought to be applied to the surrounding parts, and followed by tepid cataplasms. Some authors have recommended scarifications, but, I cannot approve of this practice; firstly, because I have seen much annoyance, and never any good arise from them; and secondly, because the principle upon which they have been recommended is erroneous, viz: that as piles are dilated veins, their puncture ought to afford much blood, and thus disgorge the vessels of the rectum. When they descend, and the surrounding parts are relaxed, we may advantageously use the ointment of galls. In consequence of pain it may be advisable to add opium, or of spasm of the sphincter, belladonna to this ointment; and should there be ulceration, and fungous asperities on their surface, the super-acetate of lead will prove a useful addition, in the proportion of half a drachm, or even a drachm, to an ounce.

“Where the spasm and pain did not forbid it, I have ordered half a drachm of the sulphate of zinc, in half a pint of water, to be injected every morning after defecation, and in the evening a steel bougie to be passed a few inches into the bowels, and kept so for half an hour. This plan has in some instances answered very well, and on the whole, appears to me much more useful than it was esteemed by those who first tried it.

“The means now stated are sufficient, in the majority of cases, to enable the individual to pass his life comfortably; but when, in spite of their judicious employment, the tumours continue to be neuralgic, attended with spasm of the sphincter, subject to protrusion, or bleed profusely, they ought to be removed. I must here, however, caution the inexperienced against precipitate determination, and this I cannot more effectually do, than by repeating, that hæmorrhoidal affections are generally constitutional, and serve to ward off fatal disease of other organs, therefore, it is absolutely necessary to interrogate the patient as to his hereditary predisposition to other disease, to the present state of the organs most essential to life, to his health previous to the formation of these tumours, and the influence they have exerted on it. Having, after a mature consideration of his case, determined upon the propriety of removing them, the patient ought to be informed of the course of treatment, which we shall specify in another place, as necessary after the operation, and without he consents to pursue it, the surgeon ought not to proceed to operation. Indeed, when we meet with a rational patient, we ought to explain to him all the circumstances relative to his case, or, if he be not a sound thinking person, it will be prudent to confer

with some of his friends ; a course not only proper in this, but in all other cases which may present themselves for operation."

Very judiciously, we think, he is a decided opponent to excision. In applying the ligature on piles, he recommends the use of several instruments of his own invention, but for which, as the description without the plates would convey but a confused idea, we must refer our readers to the work itself. There is one point in his directions for this operation, against which we must enter a decided protest ; after a needle, armed with a double three silk ligature, has been passed through the tumour, and the needle removed, he says—

"Each half of the tumour being tied as firmly as possible, all of it, save a small portion in front of the ligature, ought to be cut off with a curved scissors."

Often as we have seen this operation performed, we never saw any inconvenience from leaving the tumour after the application of the ligatures ; but it is not a week since we saw a ligature, although applied very tightly, slip off. Nothing worse than the repetition of the operation was the consequence ; but had the direction of our author been followed, and the tumour cut off beyond the ligature, hæmorrhage, the objection to excision, would probably have caused much trouble and danger.

There are other subjects of much interest, and equally well treated, which we should have wished to have considered, particularly abscess near the anus, fistula, and stricture of the rectum ; but this our want of space forbids. In reviewing the work we have not hesitated to say where we differed from the author, or to point out where we thought him wrong ; this, we are happy to say, has not been often necessary ; indeed, from the general excellence of this treatise, we cannot doubt but that it must rank high as an authority on this class of diseases. With respect to the mechanical part of the work, if we are to regard it as a specimen of American typography, our Transatlantic brethren are sadly behind us in this respect ; even at the beginning the printer has printed his own name so badly, that with much difficulty we discovered it to be James Van Norden. Dr. Bushe, in the preface, claims, very fairly, the reader's indulgence for there being many mistakes, which from his ill health he was unable to correct ; but there are *printer's* mistakes in almost every page, such as larceration, gray, dijestion, conjection, dilated sulphuric acid, &c. not to mention parts of French quotations perfectly unintelligible, on which we are not inclined to bestow the least indulgence, as such slovenly printing would be disgraceful to any press. Although so short a time in Ame-

rica, the author too occasionally falls into Americanisms :—" A gentleman came to me last *fall* ;" " I touched the surface with a swab soaked in nitric acid ;" " a *cane* of the nitrate of silver ;" &c.; but these are perhaps trifles scarce worth noticing.

We cannot bring these observations to a close, without a melancholy feeling of regret, that death should have so soon arrested the workings of a mind from which such benefits to science might have been anticipated.

J. H.

The Philosophy of Human Nature in its Physical, Intellectual, and Moral Relations. By HENRY M'CORMAC, M. D. London, 1837.—Longman.

We regret that the subject matter of Doctor M'Cormac's book is of a nature which does not permit us (consistently with the plan laid down for the conduct of this Journal) to lay before our readers a sketch of its contents, or indulge ourselves in making extracts from the work itself. Doctor M'Cormac is already very favourably known to the profession by his *Essay on Fever*, and we have no doubt that his reputation will not suffer by the *Philosophy of Human Nature*, which contains much interesting matter, and evinces great originality of mind, and deep thought.

Remarks on Mr. Aldridge's Criticism on the London Pharmacopœia. By RICHARD PHILLIPS, F. R. S., L. and E., and Lecturer on Chemistry at St. Thomas's Hospital.

TO THE EDITOR OF THE DUBLIN JOURNAL OF MEDICAL SCIENCE.

SIR,

The Number of your Journal for March last, contains a notice of the *London Pharmacopœia*, signed John Aldridge ; this performance, I presume, you inserted, in consequence of knowing that the author of it had enjoyed, at least, an opportunity of being acquainted with the subject on which he has ventured to become a critic.

I trust to your candour for inserting some remarks on this communication, relating chiefly to the chemical portions of it, observing, however, that the fantastic style of the author exhibits " the madness of poetry without the inspiration ;"—thus

"authorized codes of pharmaceutic lore" (*anglice* pharmacopœias) are "comets" which "spring forward" "like angels' visits" "to illuminate the medical horizon," and in order that they may be duly scrutinized, "every eye becomes armed with a philosophic tube," &c. &c. &c.

The effects which this critic's observations, made with his *armed eye*, have produced upon him, appear to have been of the most varied descriptions; and some of the operations to which he has been subjected in consequence of his prying, are such as few persons would willingly submit to. Thus he is by turns *amused* and *startled*, *dazzled* and *shocked*, he *laughs* and *loathes*, *smiles* and is *sick*.

To proceed, however, with the chemical observations of this critic: the first directions which incur his high displeasure, are those for preparing nitrate of silver; and after translating *acidi nitrici fluidunciam*, by *half an ounce of nitric acid*, he exclaims, "all this as far as it goes is very correct;" and it thus unfortunately happens that almost the only commendation which escapes the critic during his examination of the Pharmacopœia, is bestowed upon a blunder, and one too of his own making. After this happy commencement, he proceeds to give a long rigmarole account of how one Mr. Ferguson (of whom more anon) buys *pure* silver containing gold and copper, and how he separates the copper from the pure silver, by a process which is quite incredible; and the critic evidently does not know, that in England *pure* silver is free from copper.

The critic (meaning by this term always Mr. John Aldridge) then asserts, that nitrate of silver "does not become blackened in the sunshine;" if so, considerable pains have been taken by the authors of the Dublin Pharmacopœia to prevent the occurrence of what never happens, I refer the critic to Dr. Barker's translation, pp. 219, 221, 229, where nitrate of silver is directed to be kept in the dark, for the same reason as given by the London College; "exposure to light," says Dr. Barker, "contributes to give to the external surface of the cylinders of lunar caustic the deep purple or black hue."

The critic then says—

"The test given for the purity of nitrate of silver is perfectly fallacious, for when chloride of silver is thrown down from a concentrated solution, by an excess of chloride of sodium, some of the precipitate is always dissolved, and the supernatant liquid will consequently become discoloured by the sulphuretted hydrogen."

Now this is a fair specimen of the critic's foul misrepresentation: he represents the College as ordering a *concentrated* solution of nitrate of silver to be used with *excess* of chloride of sodium, well knowing that no such directions are given.

The operations which I shall next notice as incurring the censure of this critic, are those for preparing protoxide of mercury; and he has the hardihood to assert, that when chloride of mercury and lime-water were employed by him in the proportions directed by the College, and he applied heat to a portion of the product, "he was edified with the spectacle of five-sixths subliming in the form of protochloride of mercury." I have made this experiment also, and in the mode which, I presume, the critic would have adopted had he performed it—that of attempting a failure. With this intention I shook a drachm of chloride of mercury on a pint of lime water for less than a minute, and immediately separated the precipitate by filtration; the solution contained excess of lime, and after saturating this with nitric acid, I added nitrate of silver, and obtained 35.3 grains of dry chloride; the whole quantity which the chloride of mercury could have yielded, would have been 36.3 grains, making no allowance for the error of experimenting. Admitting for a moment, that the critic did make the experiment, it is evident that he put no faith in what he has stated to be the result of it; for if he had, he would not have hesitated to declare that the College never tried whether the precipitate was soluble in acetic acid or not, instead of merely expressing a doubt of it. I have, however, no hesitation in asserting, that the critic never tried the College process at all, or did not obtain the results which he describes, and by which he was "edified."

The critic then considers the directions for preparing acetic acid, which he instances as one of "those preparations, which have not the slightest use in medical practice." Even the critic himself never made an assertion more directly opposed by facts; the acetic acid is used in preparing *potassæ acetæ*, *plumbi acetæ*, and *oxymel*. The process answers the purpose perfectly well, whereas that proposed by the critic is objectionable in every respect: the product is small in quantity, bad in quality, not very strong, two distillations are required, and the residue is with difficulty got out of the retort.

The critic states, that when trisnitrate of bismuth is prepared as directed by the College, loss of metal is incurred, and he is certainly right in this respect, but so unused is his pen to fact, that it returns to fiction naturally and immediately; for the critic asserts, and without the slightest ground for it, that the trisnitrate of bismuth obtained by the College process is discoloured.

A general charge is then brought by the critic against the College; he tells us, that "new preparations are substituted for old ones, with the most reckless indifference to the opinions of the profession." To support this statement he adduces a pre-

paration which proves precisely the reverse, and it is exhibited with as great a parade and profusion of blunders, as ever were displayed. The critic says, that in preparing what he calls the "old tartar of iron," by exposing iron to the action of bitartrate of potash, water and air, "the iron, influenced by induction from the potassium, only acquired a proto-state of oxydation." From this statement it is evident that the critic is ignorant of the fact, that when iron is acted upon by bitartrate of potash and water, the oxygen which the metal first acquires is derived from the decomposition of the water, the hydrogen of which is evolved in the state of gas. I refer the reader to p. 254 of Dr. Barker's Translation, where he will find the true source of the oxygen mentioned. The critic says, that by the process just described, "there was obtained a permanent protosalt;" thus an error in theory is naturally followed by a blunder as to fact. It is well known that tartarized iron contains the sesquioxide, and not the protoxide of iron, the additional portion of oxygen being acquired from the air. Dr. Barker will again set the critic right if he will refer to p. 256 of his Translation, where he says, that when this preparation "is heated with potash nearly to ebullition, peroxide of iron separates abundantly;" thus our critic grossly misstates the source of the first portion of oxygen, and errs as greatly in denying the existence of the second.

I have not, however, yet done with the "old tartar of iron," for the critic states, that "the whole preparation is worthy of notice, as a precious example of pharmaceutic legislation." Thus do arrogance and ignorance proceed together. The process is this: the ferri subcarbonas of the late Pharmacopœia, is replaced by the ferri sesquioxylum of the present; they differ but little, except in name. A portion of this substance is directed to be dissolved in hydrochloric acid, and to be precipitated from the solution by potash. The moist, hydrated, sesquioxide of iron thus obtained, is to be dissolved by boiling with bitartrate of potash in water; the critic, ignorant of the control exerted by cohesion over the solvent power of acids, thinks the dry sesquioxide of iron as well adapted for solution as the moist hydrate. The fact, however, is, that when the former is used, bitartrate of potash takes up only about one-tenth of its weight; whereas of the latter it dissolves nearly one-fourth. If the critic doubt the accuracy of my assertion, let him refer to p. 255 of Dr. Barker's Translation, where he will find it corroborated in general terms. The present process, with slight alteration, is taken from Soubeiran.

Having stated that some preparations are improper, for reasons which he has assigned, the critic makes the sweeping declaration, that—

“There are several which are objectionable in every point of view; tartar emetic is made by boiling cream of tartar with crocus of antimony.”

And he then quotes a passage from Berzelius, which has nothing to do with the process; for I assert, in direct contradiction to the critic, that crocus of antimony is *not* used by the College. Crocus of antimony is prepared by mixing, deflagrating, and fluxing a mixture of equal weights of sesquisulphuret of antimony and nitrate of potash, and the crocus is separated mechanically from the saline matter. In the College process the same ingredients are employed, mixed, however, with some hydrochloric acid; this mixture is merely deflagrated, and the saline admixture is separated by washing. In this way a preparation of antimony is obtained in a state of aggregation, much more favourable for solution than the crocus obtained by fusion, and which it is difficult, also, perfectly to separate from the saline matter. I do not apprehend that the critic will be able to appreciate these advantages, but those who are in the habit of making experiments otherwise than upon paper, will be able to judge of them.

Iodide of potassium is directed to be obtained by decomposing the iodide of iron with carbonate of potash; this, says the critic, is

“Baup’s process, which is objectionable from the difficulty of avoiding an excess of potash on the one hand, or of suffering a considerable loss on the other, from the precipitate of peroxide of iron carrying down a large quantity of iodine.”

Now Soubeiran, a well known French author, whose opinion, greatly, I have no doubt, to the surprise of the critic, I shall prefer to his, says “*Le meilleur procédé pour obtenir ce sel est celui de Baup et Caillot.*”^{*} I assert that there is no danger of excess of potash when due care is taken; and as to the precipitate of peroxide of iron of which the critic talks about, there is none; and so deplorably ignorant is he of the facts of the case, as to suppose that when proto-iodide of iron is decomposed by carbonate of potash, the precipitate obtained is sesquioxide, instead of proto-carbonate of iron. No waste of iodine, however, occurs from precipitation with the proto-carbonate of iron; I found that within 1-160th of the whole

^{*} *Pharmacie*, tome ii. p. 374.

theoretic quantity of carbonate of potash was required to precipitate the iron in the state of proto-carbonate; this would not have been the case if the iodine had been precipitated with it, instead of remaining to combine with the potassium of the carbonate of potash.

"Our friend Mr. Ferguson," he who uses pure silver containing copper, I presume, is now cited by the critic, and for the purpose of exhibiting a series of chemical wonders. The passage which contains them is not very long, and is much too curious to be abridged.

"Mr. Ferguson," says the critic, "has mentioned to us a process which he has been in the habit of pursuing, which is simple, cheap, and most extraordinary: by boiling carbonate of potash, iodine, and iron turnings together, with a proper quantity of water, iodide of potassium is formed, carbonic acid and oxygen escaping with a violent effervescence, and the iron turnings remain untouched. This is most strange, and evidently belongs to the operations of catalysis, lately generalized by Berzelius."

Now by referring to Berzelius, it will be seen that this power is not exerted, when action can take place without it, and that the substance which produces the action remains unchanged. We have then first to inquire, whether when iodine is heated in a solution of carbonate of potash, any action takes place between them, and any one who will try the experiment will find, as I did, that the iodine is dissolved, and the carbonate of potash decomposed with the evolution of carbonic acid gas; this combination, therefore, is not the result of catalysis. In order, however, to try what effect would be produced by the presence of iron, I boiled in a retort, 252 grains of iodine and 56 of iron, in a solution of 177 grains of hydrated carbonate of potash. Carbonic acid gas was plentifully evolved, but not a particle of oxygen gas was evolved from the commencement to the close of the operation. Instead of the iron "remaining untouched," nearly one-half of it was converted into sesquioxide. I pronounce, therefore, the whole statement respecting this "most extraordinary" process to be a tissue of absurdities.

With this critic every word is to be twisted as much as possible, in order that it may not mean what was intended: thus, as already shown, with respect to nitrate of silver, he represents the solution as directed to be concentrated, and chloride of sodium to be in excess, in order that they may not produce the desired and described effect. So also when it is stated that iodide of potassium loses nothing when subjected to heat, according to the critic's version, the heat is to be of that degree which is well known to volatilize it.

Chloride of barium is directed in the Pharmacopœia to be prepared by decomposing carbonate of barytes with hydrochloric acid. "Every one knows," says the critic, "how much more rare a mineral witherite is than heavy spar." The critic, however, does not appear to know, that in the same country as that in which pure silver contains no copper, carbonate of barytes may be bought for about one penny per pound; and if the sulphate cost nothing, it is more economical to employ the carbonate.

"Sulphate of potash," says the critic, "the College order to be made by roasting the supersalt remaining after the preparation of nitric acid, until the excess of acid is separated. This is a most awkward method; besides that this residual salt is much sought after in the arts."

More blunders: this salt is not much sought after in the arts, and very little of it could be had if it were, for the quantity made is extremely small; the common residual sulphate, containing but a slight excess of sulphuric acid, is preferred, whether intended for alum making, or conversion into carbonate. Nothing can be more ridiculous than the mode of preparing sulphate of potash, propounded by this grave critic, except the reasons for preferring it. It is too bad to copy.

In the Pharmacopœia iodide of mercury is ordered to be prepared by triturating together the requisite quantities of the ingredients, and adding a little spirit during the operation.

"We," the critic, "prepared some in this way, and mixing it with a solution of chloride of sodium, allowed it to filter; upon the addition of nitrate of silver, a copious precipitate was thrown down, which was only partially dissolved in water of ammonia. The iodide of mercury evidently contained a large quantity of biniodide."

I have no doubt the critic will again think it strange, but I must again declare my preference of Soubeiran's opinion in favour of a process, to the experiments of so great a blunderer against it. Soubeiran says, that the process which the College has adopted is the best; and in direct contradiction to the fallacious experiments of the critic, (if indeed he performed any,) I assert that the iodide of mercury, prepared as directed, was so perfect, that sulphurated hydrogen added to the solution of common salt on which it was triturated, gave not the slightest precipitate. It was easy for the critic *not* to add sufficient ammonia to dissolve the chloride of silver which he precipitated,

"An oxide of iron," continues the critic, "containing twelve or fifteen per cent. of proto-carbonate, has not hitherto

been considered as a very pure sample of sesquioxide;" certainly not, but Mr. Ferguson would perhaps admit it to be so; the sesquioxide of the Pharmacopœia, contains, however, scarcely one per cent. of it; but when it suits the purpose of this consistent critic, he can make a very different statement; thus he says in another place, "that what is commonly called the precipitated carbonate of iron is itself a sesquioxide."

I have now concluded my remarks on the lucubrations of the critic; were I not disgusted with the occupation, I might greatly have extended my observations, and have shown, by way of variety, the havoc he has committed with proper names, and with quotations; but I am tired of wading through so contemptible a production, and which originated either in the inveterate ignorance, or wilful and malignant misrepresentation of its author.

I am, Sir,

Your obedient Servant,

R. PHILLIPS.

SCIENTIFIC INTELLIGENCE.

GLEANINGS BY DR. BIGGER.

Umbilical Hernia, with escape of Urine through the unclosed Urachus.—The following case, observed by Dr. Schmidt of Creisswald, is taken from the official reports of the Royal Medical College of Pomerania.

In an infant, aged four weeks, who laboured under umbilical hernia, a considerable discharge of fluid from the navel was observed. On examination it was found that the contents of the sac had passed about three quarters of an inch out of the umbilicus; at its summit there was an excoriated spot, about a quarter of an inch in length, and a line in breadth, from which a considerable quantity of fluid dropped, having a strongly urinous smell. When the part was well dried the fluid was seen to ooze out distinctly from several points. The quantity was very considerable, for a pellet covered with leather, which had been applied over the part for twenty-four hours, was so thoroughly soaked, that it appeared as if rotten, and the compress and bandages were quite wet. The natural passage for the urine was normal. The hernia was reduced, the excoriated spot covered with charpie, a smooth piece of cork laid over this and fastened with strips of adhesive plaster, a compress and bandage. After twenty-four hours, the exudation of fluid ceased, and at present the rupture is completely gone.—*Medinische Zeitung.*

Diphtherite—The child H., 8 years old, of scrofulous habit, and who had already been suffering in her chest for three years, complained of loss of appetite, some pain in swallowing, and great weakness, occurring in consequence of catching cold in the latter end of June of this year; notwithstanding she continued to attend school. Her parents considered this illness as trifling, and first sent for me on the 2nd of July, as the ordinary household remedy for sore throat had been found of no avail.

I found the child suffering much, and looking very pale, the eye heavy, the skin cold, respiration somewhat hurried, but in other respects unimpeded, and the voice not altered; there was no cough; the pulse quick and small; the urine clear and a brilliant yellow; and the evacuations free and of ordinary constitution; her appetite had failed entirely, no great thirst, and for some nights the little girl had hardly had any sleep. It was only in the neighbourhood of the

slightly enlarged submaxillary gland, that the neck was uneasy to the touch. From her nose and mouth a nauseous unpleasant odour proceeded; the tongue was covered with a yellow mucus, and the hard and soft palates were paler than in the natural condition; the uvula was covered with a firmly attached, whitish-yellow, tenacious mucus, the tonsils manifestly enlarged, and covered with a white, lardacious, fibrous mass, which could be torn off by the forceps, in small strips, which smelt very offensively, without giving any pain. On the posterior wall of the pharynx were three insulated spots of the size of a pfenning, (about as large as an English silver four-penny-piece,) covered with a similar mass. By this the deglutition was rendered difficult, but not at all so much as is the case in angina.

The evident cachectic condition of the patient, and tendency to putridity, forbade any violent antiphlogistic treatment, and determined me to employ internally the aqua oxymuriatica, lemonade as drink, and to touch the parts affected with a camel's hair pencil dipped in muriatic acid and syrup of mulberries, and I ordered the neck to be rubbed with the linimentum ammoniæ.

On the 3rd of July the girl, after a sleepless night, was to all appearance altered for the worse; her eyes looked very heavy. Her pulse was quick, small, and weak, her respiration not difficult, but hurried; her voice somewhat rough; there was no cough, nor any sympathy of the larynx or trachea, but the lardacious coating had acquired a much greater circumference; the uvula and the neighbouring parts to the soft palate, and the parts about the tonsils, particularly behind and beneath, were covered with a white exudation; two of the insulated spots at the back of the pharynx had become continuous: the Eustachian tube appeared also to be engaged, for the child complained of pain deeply seated in the ear, particularly on opening the mouth. The offensive odour from the nose and mouth was still worse than on the day before; the swallowing of fluids and soft matters went on tolerably well.

As what had been applied with the pencil had produced no change, I caused pyroligneous acid mixed with syrup to be applied to the lardacious envelope, and in the intervals of its application, a gargle of aqua salviæ, acidum muriaticum, and syrupus mororum, to be used.

4th July. Great falling away; features much changed; visible emaciation; cadaverous smell from the mouth; and still greater extension of the lardy coating on every side. The Eustachian tubes were, to judge from the pain, still more and more affected, as also the posterior openings of the nares.

I now ordered a decoction of red bark with dilute sulphuric acid, to be taken internally, and a solution, first of two grains of nitrate of silver to the ounce of water, and afterwards, as that had produced no effect, three grains, to be applied locally. The child got, besides, meat soup with yolks of eggs, and a little red wine frequently. The greatest attention was paid to give her fresh air, and to keep her cool.

On the 5th her condition was still more melancholy, her general appearance very bad; her pallid countenance fallen, and painfully drawn; her complaints of pain in the inner ear and nose very great; the voice hoarser, but the respiration and deglutition not very difficult; the smell hardly endurable, so that the window was obliged to be kept constantly open, and chloride of lime to be exposed in plates; the pharynx as far as could be seen, the tonsils, uvula, soft palate, and inner surfaces of the cheeks, were covered with a thick, lardy concretion; this had been rendered brownish by the lapis infernalis, and I succeeded in separating a piece with a forceps, of the size of a silver groschen, (about that of a British six-pence,) from the posterior wall of the pharynx, without causing bleeding or pain. This false membrane was more than a line in thickness, of firm homogeneous consistency, and only to be torn with difficulty. No blood-vessels were discoverable in it. The place from which it was separated was of a bluish brown, very soft, and had evidently passed into a state of gangrene; in the course of the day many and smaller pieces of this membrane became detached and were thrown off; also in the thin, brownish green, and extremely offensive stools, were similar shreds to be seen. In the evening the greater part of the exudation had disappeared; not so the fœtor: I had added to the decoction of bark an infusion of the root of serpentaria, in small doses, and ordered the cavity of the mouth to be brushed with the solution of nitrate of silver: I had hopes of being able to arrest the progress of the gangrene in the parts freed from the exudation.

On the morning of the 6th the exudation which had been cast off, was renewed by a similar one; the pain in the ears was unendurable; the breath, as before, offensive; the respiration difficult, and the voice very weak and hoarse; the larynx and trachea were not painful, but to judge by the voice and respiration, they must have been covered by the exudation. Her strength had completely sunk, and her skin, particularly at the extremities, was cold; pulse small, empty, and hardly to be counted; her pale countenance bore the expression of the intensest suffering.

I prescribed the decoct. cinchonæ with infus. serpentariæ and acid. mur., and at intervals small doses of camphor, red wine, and sprinkling of the mouth and cavity of the jaws with aqua kreosoti. No means could now counteract, or in the smallest degree impede, the course of this terrible disease; it advanced irresistibly.

In the evening this poor girl, constantly pointing to her head, and particularly to her ears, presented, being perfectly in her senses, a most miserable picture, with her pale, painfully-drawn visage, and her wearied and weak voice, which, however, she seldom employed; at this time the atmosphere around her, despite of all antidotes, was absolutely pestiferous. Death, which was now wished for even by her parents, was every moment to be expected; it did not arrive, however, till the following morning, after a miserable night. Unfortunately I could not obtain an examination of the body.

I was more fortunate in the cases of two brothers and a sister of this little girl, whom I had caused to be separated from her already on the 2nd of July, and for whom I had ordered acids and plentiful exercise in the open air. On visiting them daily, I found that on the 4th the two brothers respectively of five and six and a half years of age, had their tonsils swollen, and covered with the iardacious exudation, without pain in swallowing or inflammation having preceded. Their breath was offensive, their tongues thickly overlaid with yellow mucus, and as signs of gastric derangement, there were present eructation, sickness, and sense of weight about the cardia. I gave them at once an emetic, and brushed the amygdalæ strongly with the nitrate of silver. After forty-eight hours a thick but soft coating was thrown off from the tonsils; they were of a bright red, and had on their surface a thin coating of tolerably healthy pus. By the internal use of the oxymuriatic acid water, and a gargle of alum dissolved in aqua melissæ, the boys perfectly recovered in eight days. Their sister, a girl of three years, taken ill two days after the boys, was treated in a similar manner and recovered.

These four cases, which I had observed from the commencement, serve as confirmations of statements hitherto made, that exudation and false membrane can be formed with incredible rapidity, without any inflammation having preceded. Whether this disease be contagious, which the taking ill of the four children of the same family would appear to evidence, is a question which, from the small number of cases, I could not attempt to answer; I would remark, however, that Bourgeois, who made observations on the diptheritis of the years 1827 and 1828, in St. Denis, as occurring in fifty-seven individuals, of whom five died under similar symptoms to those which I have detailed above, has declared his opinion that it is not contagious.—*Mayer in Kreutzberg.*

The similitude of the cases detailed above, to those described by Dr. Bewley, in the January Number, (No. XXIV.) of the year 1836, of this Journal, is most striking, almost in all points, and although the treatment pursued by Dr. B. was much more active than that of Mayer, still the diptheritis held its course irresistibly till death closed the frightful scene.

Results of Re-vaccination in the Royal Prussian Army, in the Year 1836.

In 1836, in the different divisions of the whole army, there were vaccinated	42,124
Of this number there were possessing marks of former vaccination—	
Distinct,	32,635
Indistinct,	6,645
Residue having no marks at all,	2,844

Took the infection, (being regularly examined during its course,)—	
Regularly,	18,136
Irregularly,	9,940
The vaccination was without effect in	14,048
Those in whom the vaccination had been ineffective, and on	
whom it was repeated, successfully	1,569
Unsuccessfully,	8,205
By vaccination true pustules were produced—	
From 1 to 5 in 7,311	
6 to 10 in 5,647	
11 to 20 in 4,418	
21 to 30 in 760	

Amongst those successfully re-vaccinated in 1836, and earlier, there occurred in the course of that year—

Of varicella	14 cases.
Of varioloid affections	8
Of true variola	0

The military duties which the troops necessarily had to perform, and the want of vaccine matter, very often prevented re-vaccination during this year; and the same reason prevented the repetition of vaccination in those cases where re-vaccination had been unsuccessful. In by far the majority of cases the zeal of the military surgeons overcame those difficulties, as an inspection of this paper plainly shows.

The vaccination succeeded, as in former years, as often as it was performed with fresh lymph from true vaccine pustules of children who had been vaccinated, and when the infection was transferred from arm to arm. In many divisions of the army, all the recruits were vaccinated in this manner. In others, however, this source was less attainable, and a greater or less portion of the men had to be vaccinated with lymph taken from genuine vaccine pustules, in adults who had been vaccinated or re-vaccinated, and which pustules were running a regular course, and thus the infection was carried from arm to arm.

All the military surgeons who have tried the latter experiment for the propagation of cow-pock, attest with one voice that it has been attended with the best consequences, because in this way, equally good pustules, which ran as regular a course, were produced, as in those cases in which the lymph for vaccination had been taken from the true vaccine pustules of children, and carried from arm to arm.

Dr. Bonardin, military surgeon, from his observations during this and former years, is disposed to agree with the Wurtemberg physicians, who give the preference to lymph taken from the true cow-pock of adults, to that taken from the pustules of vaccinated children.

Dr. Hering, surgeon of battalion, as an experiment, had vaccinated a portion of the men appointed to be vaccinated, on one arm with lymph from children who had been vaccinated, and on the other

arm, with that from the true vaccine pustule of adults, and has seen particularly favourable results, as these persons have had a remarkably good and well-formed pock in consequence; it was also good in those individuals vaccinated with lymph from children, but the pustules were better developed in those vaccinated with lymph taken from adults.

Lymph preserved for infection has been used only here and there by the military surgeons during this year, when they were obliged to have recourse to it from the want of fresh vaccine lymph. Drs. Schwarz and Bonardin employed the lymph sent to them, on slips of wood for this purpose by Dr. Bremer, from the Cow-pock Institution here, with the very best consequences.

In the application of this dry lymph, Dr. Schwarz wets it with a little saliva, which he esteems particularly well adapted to make it fit for vaccination, without weakening its powers of propagation.

Generally from twenty to thirty scratches were made this year, divided between both arms. This number was seldom exceeded; and often, for want of vaccine lymph, we were obliged to be contented with a much smaller number.

In the majority of cases the course and form of the pustules was regular, and there was nothing to distinguish them from the genuine pustule of vaccinated children.

In those cases in which the inoculation produced numerous pustules, generally there was more or less swelling of the axillary glands, and sometimes there was a general feverish reaction, which particularly accompanied the stage of maturation. Occasionally during this year the remark was made, that vaccine pustules of the true form appeared one, two, or three days too late, but from that time they fulfilled their normal period, so that in such cases, the stage of repletion and suppuration came on, delayed by one, two, or three days. This species of late pock, in which the inflammatory halo first came on with swelling of the skin on the twelfth day after the inoculation, was observed very often in the 15th Regiment of Infantry; and those late pustules, which were not regarded as true cow-pock by Dr. Bonardin, always remained small. Particularly numerous cases were seen by Eichorn in the same year, in a form which he denominated "modified cow-pock;" in them the disease developed itself very quickly, the pustules had an inflammatory redness surrounding them, and from the fifth day, only displayed a tubercular appearance. Although all those cases by Dr. Bonardin (the number in the 15th Regiment amounted to 327) had plainly pursued "an irregular course," still he regards these modified vaccine pustules, with Eichorn, (an opinion sufficiently bold,) as vaccine, inasmuch as none of those individuals were affected with natural smallpox. In some cases the course of the disease was complicated by other accompanying complaints. In one case in the 21st Regiment of Infantry, there was inflammation of the lungs which occurred, and ran its course to recovery along with the cow-pock: this came on after the inoculation, when twelve genuine pustules had been formed. In two other cases smallpox formed the

complication. In a recruit belonging to the same corps, who bore evident marks of former vaccination, and who was now re-vaccinated successfully, true smallpox occurred. The pustules of both pocks ran their usual course in juxta-position, without being the least disturbed in their individual development. The second case of this kind occurred in an individual belonging to the fusileer battalion of the 31st Infantry, who on the 13th of May had been re-vaccinated, and in whom true smallpox occurred on the 22nd of May, after eighteen true pustules, the consequence of the vaccination, had appeared.

A fusileer of the 3rd Infantry Regiment, who had already in 1835 been re-vaccinated successfully, was in 1836 once more submitted to re-vaccination, and even then with success; in 1835 he had fifteen, and in 1836, twenty-four true cow-pock pustules formed as a consequence of the re-vaccination.

In this year also examples were not wanting of successful vaccination of persons who bore more or less manifest marks of smallpox having occurred at some former period. No case occurred of any person remarkably pitted with smallpox, having been a second time attacked with the natural smallpox in the army during the course of this year. In some cases it again occurred as in former experiences, that where the marks of earlier vaccination were very fine, large, round, and punctuated, that re-vaccination again produced very perfect and large pustules; yet this occurred more frequently where two or three incisions had been practised, than where six or seven had been made.

Other military surgeons, on the contrary, support the opposite opinion, inasmuch as they cannot regard manifest marks in those persons who have been re-vaccinated, amongst the causes why the re-vaccination should be without consequences.

It is on account of this reason of the want of success in re-vaccination, that it was determined, in the 5th corps of the army, this year also, that those persons who had been a short time before re-vaccinated in the civil re-vaccination, instituted in the cities, if they cannot show the cicatrix of the pustule, should at their entry into the army again be re-vaccinated. However, cases of this kind occurred less frequently than in 1835, because the greater number of those persons brought attestations of re-vaccination having been performed on them.

The present inspection shows, that from the 42,124 persons vaccinated and re-vaccinated in 1836, 18,136 had genuine vaccine pustules running their regular course. This exceeds by much the results of former years. For example, in the year 1833, 48,478 persons were vaccinated, of whom were 15,269 with success. In 1834, 44,454 vaccinated, of whom 16,679 with success. In 1835, 39,192 were vaccinated, of whom 15,315 with success. The proportion is almost this, that in—

100 vaccinated in 1833 it succeeded in				30
Do.	do.	1834	do.	37
Do.	do.	1835	do.	39
Do.	do.	1836	do in almost	43

The inoculation producing cow-pock running a regular course. This proportion has hitherto been increasing: whether it will in time to come proceed to any certain limit, the evidence of next year must tell. As above all this increasing proportion of successful inoculation cannot be caused by mere chance circumstances, so we may infer with probability, that there is an increasing capability for the reception of the vaccine infection in the yearly levies, with which, however, according to our former experiments, an increased predisposition for the contagion of smallpox must be received at the same time. The ever-increasing cases of smallpox in civil life, and the frequent attacks of it amongst the recruits, invariably just after their joining, appear to justify these inductions, which perhaps might be confirmed by the results, if bounds are not set to the further extension of smallpox in the army by the re-vaccination of the recruits. In this respect the re-vaccination in the army this year, has given very satisfactory results. In 1836, of those who had been re-vaccinated then or earlier, twenty-two were taken ill; eight affected with varioloid disease, and fourteen with varicella; and these diseases in all ran a very mild and moderate course. In the 6th Curassiers, however, one case of genuine variola occurred, which terminated fatally; this man bore evident marks of early vaccination, and had been revaccinated, inasmuch as good pustules had formed as a consequence of re-vaccination: the man had rubbed these off afterwards, upon which a protracted and considerable suppurative inflammation in the arm came on. By this means the vaccine pustules were disturbed in their course, and could not be considered as vaccine; wherefore this man is not included in the number of re-vaccinated with success.

In the year 1836, 130 cases of natural smallpox (true, modified, and false pock) occurred in the army, amongst which the twenty-two mentioned above were included. In the majority it was varicella and varioloid disease of a mild nature, and only about one-fourth suffered from the true smallpox, in which, including the fatal cases mentioned, nine deaths occurred, of which six happened in January.

The greatest number of those taken ill of natural smallpox was soon after the junction of the new levies to the troops, before it was possible to submit them to re-vaccination. Some of them took ill already upon the march, or immediately after having reached their respective troops. As soon as the revaccination of the recruits was over, the smallpox ceased to commit its ravages amongst the troops, with very few exceptions, notwithstanding that in many imperial garrisons, as Mainz and Luxembourg, smallpox prevailed to a greater or less extent amongst the civilians, and was the cause of much fatality. In Weisenfels, and in the district around, already since the month of March, varioloid disease, and varicella, had prevailed amongst the inhabitants, when in the months of April and May, seven persons, all recruits, also of the 31st Regiment of Infantry, there in garrison, were attacked with the natural smallpox. The first, a recruit of the 12th company, took ill on the 22nd of April of varicella,

and four others of the same company quickly followed ; they all slept in the same room. Of these, three had varicella ; but in the fourth, true smallpox developed itself, which soon became confluent, and ended in death. On the 9th day of May, a man who was in the lazaretto on account of some wounds, was attacked with true smallpox, and soon after another, who had been already in hospital fourteen days, for a gastro-catarrhal fever, got varicella.

The whole seven recruits had been vaccinated in their youth, as the cicatrices upon their arms clearly showed ; re-vaccination had not, however, been performed on them, because the battalion surgeon, Lohse, busied with the re-vaccination, which he had commenced in the month of April, had not yet been able to get to the 12th company, it being the last in order. When the re-vaccination had been perfectly accomplished in the battalion, not a single man more became affected with smallpox.

In 1836, one case of varicella was received from the 7th Corps, which occurred to a volunteer of the 3rd battalion of the 15th Regiment, Landwehr, who had been vaccinated in his youth, and again re-vaccinated in 1835 successfully. Not a man was taken ill of the natural smallpox, although it prevailed amongst the inhabitants of many garrisoned places in the neighbourhood of this corps of the army.

In conclusion, it is worthy of remark, that this year more cases have occurred of individuals being taken ill of true or modified smallpox, on whom re-vaccination had been tried once or twice without effect.—*Collected from the General Report of the Medical Staff, Berlin, by Dr. Lohmeyer.*

Local Manifestations of Intermittent Fever—The wife of a citizen of this place (Mühlheim on the River Rhuhr) of a very delicate constitution, and hectic make, whose life had been preserved by an attention to diet which hope and the love of life, so common in the hectic, alone could render endurable. She had been for many years the subject of medical treatment, particularly in Spring and Autumn, at which times she felt herself oppressed with a dry cough, accompanied by stitches in the side. When the period of puberty arrived, she suffered much from irregular menstruation, which came on sometimes in too great abundance, sometimes in too small a quantity, sometimes too early, and sometimes too late ; this impaired her strength very much, and caused congestion to the head and chest.

The approach of her courses announced itself, for many years, by a painful prickling sensation in the right hip and right arm, to which a disabled condition of this side usually succeeded, but disappeared again on the appearance of the catamenia.

These sufferings, to which she was now accustomed, returned in the Autumn of the year 1836, with greater violence than ever, and continued throughout the entire winter, on account of difficult men-

situation being superadded, and as yet (May, 1837) that state has not been entirely removed.

In the month of February of this year, at the period when influenza raged, this woman was also attacked by it. After a few days the disease changed into a very extraordinary quotidian fever, which developed itself first in the left, and afterwards in the right arm. When the patient related to me this unusual affection, I was not a little astonished at the occurrence, and that I might not be deceived, I carefully put many questions to her, and caused her to tell her complaint many times over, before giving her my opinion.

The paroxysm displayed itself in the following manner. In the morning, between 10 and 11 o'clock, soon after taking her breakfast, she had an inclination to yawn frequently, followed by a trembling of the left arm; by degrees the left arm, from the acromion to the finger points, became colder and colder, and at last as cold (as the patient expressed it) as if her arm had been immersed in cold water; it also exhibited the appearance known by the appellation of goose-skin, and the nails and fingers became blue.

This local rigor was sometimes so violent, that the arm was sometimes involuntarily jerked from one place to another: when it had lasted about an hour, it passed into the hot stage, which lasted fully two hours; after this an abundant perspiration followed, which extended itself in a modified degree, some time after, over the entire body. The first two attacks of this local fever were slight, and not much noticed by the patient, but the third was so violent that she was greatly alarmed.

I visited her at the feverish period, and saw the rigor, goose-skin, blue nails, and violent perspiration of the left arm; no part of the rest of the body was in the slightest degree affected. To my great astonishment I found that there was no difference of temperature in the two arms; but the pulse displayed a very remarkable difference; for whilst in the left it was 90, hard, wiry, and small, that in the right was only 80, and soft. In the hot stage there was no remarkable difference in the pulses, and it was only in the sweating stage that that of the left wrist was accelerated, whilst that of the right continued as in the preceding stages. In the forenoon the urine was natural, but in the afternoon it contained a lateritious deposit. In other respects there was not anything in the condition of the patient which seemed to depend in the slightest degree on the local fever in the arm.

When this person had taken sulph. quinae for two days in small doses the attack ceased, and she believed herself at length freed from her wearisome guest, when after three days her right arm began to be affected with the fever as the left had been before, and these attacks were five times repeated before they were entirely set aside by the use of quinine. Now, however, there occurred towards three o'clock in the afternoon, after a slight rigor, with transient flushes of heat over the whole body, and particularly a burning in the

hands, a general enfeebling perspiration, which was, however, gradually removed by the employment of adequately powerful means; and evidently these were rather to be considered as symptoms of the former cachectic condition of the patient, than as having any real connexion with the recently occurring local fever.

The fever described bears with it all the recognizable symptoms of a local fever, without exhibiting itself in any way as a masked fever. That there is an evident difference between a local and a masked fever seems put beyond all doubt by the case described.*—Dr. J. H. Leonhard, *Muhlheim on the Ruhr*.

Sweating of Blood from the Feet in Tertian Fever.—Dr. Kaminsky has communicated a very curious case of this affection.

The father of a girl in the country, who was suffering from tertian fever, was alarmed, not only at the fever, but particularly at a transudation of blood from the feet from one day to another. On an accurate inquiry, it appeared that the girl, who, from description, had been of a strong and plethoric habit of body, had never menstruated; the blood flowed in considerable quantity on the days of the fever, with the accession of the hot stage, from both feet, from openings caused as if by leech bites, and again ceased gradually after the paroxysm was past. By an appropriate treatment, particularly by the employment of quina, which this physician gave in combination with carbonate of magnesia, carbonic acid, and rhubarb, the fever soon disappeared, and along with it the bleeding at the feet. After pediluvia had been employed for some time, menstruation appeared.—*From the Provincial Returns of Health of the Royal College of Medicine, Pomerania.*

Frictions with Lard, by Dr. Hoffmann.—The newly prescribed frictions with lard do good service, not only in profuse and colliquative perspirations, (particularly at the upper part of the body,) but have also been found of the greatest use to other hectic subjects, whose skins were not particularly active, as in various kinds of asthma.—*Medicinische Zeitung in Preussen.*

On Toothach from Caries, by Troschel.—This author has followed up some observations made by him last year in a Prussian Medical Journal, in which he endeavoured to prove that the violent pain which occurs in caries of the teeth is not caused by the laying bare of the nerve; and that caries, if unaccompanied by any other ailment, is in most cases free from pain. There are exceptions, however, to this rule which are not uncommon.

* Whilst engaged translating this article, Dr. Graves mentioned to me, that a nearly parallel case had just occurred to him in another disease, viz. jaundice, in which the yellow tint had alone appeared on the hands, all the other parts of the body being as fair as usual.

We find ordinarily two or more carious teeth together, of which very often one gives great pain, and the others which are much more injured, and in an apparently worse condition, give no pain. Despite of all palliatives, and all possible attention in the avoidance of cold, the pain often lasts whole weeks with increasing or decreasing violence; there is congestion and repeated swelling of the face, sleep and appetite are banished, and even the good constitution of the sufferer begins to be affected. After the tooth, the author of all this suffering, has been drawn, all complaints cease, and the patient soon recovers.

If the extracted tooth be now broken in two, or what is better, sawed longitudinally through the centre, we find that from the carious part, which is often very distant from the nucleus, there extends a black or brown streak into the cavity of the tooth where the nerve lies. Sometimes this streak is not very distinctly marked, and in this part the substance of the tooth is only a little less white, duller, and more pellucid than the surrounding structure. This change of colour occurs on this account, because that the canals in the substance of the tooth, which lie in layers close one behind another, and pass from the circumference to the centre, are permeated with pus, (according to the examinations of Purkinje, Valentin, Gurlt, and Muller;) they are denominated by the last mentioned author, "*caniculi chalicophori*." In caries of the crown of the tooth, the phosphate of lime which is contained in these canals is absorbed, and during the suppuration, the carious matter infiltrates still farther from the base of the abscess into these little pores: then not only the white colour is lost, but the nucleus of the tooth (the nerve of) becomes affected, and this causes the most intolerable pain.

Every dentist of observation has seen those dark streaks which pass to the nerve; the little canals can, however, only be seen under the microscope, and then only on thin sections of the tooth prepared on a grinding stone.

It is only from very acrid applications, and such as for a period paralyze the nerve, that any alleviation is to be obtained from the torture one suffers, and which arises in the manner we have described. Even the application of the actual cautery to the carious hollow, has no lasting effects, and the extraction of the tooth remains as the only resource.

Treatment of Scabies, by Dr. N. Meyer of Minden.—On account of the great quantity of scabies which has come before us, we feel called upon to give the results of the treatment pursued on the plan recommended by Dr. Vesin for the cure of this disgusting disease; we do this the more willingly, as this plan appears to us to insure quick, cheap, and safe results.

The treatment made use of in the hospital is as follows. The patient is placed in a chamber appropriated to scabies, which in winter as well as summer is kept at a temperature equal to 28 to 30 of Reaumur; he is put in a warm bath, in which his whole body is

rubbed with black soap and coarse woollen cloths, so strongly, that all the pustules which have appeared are rubbed off. He is then put to bed between two blankets, wrapped up in a thick woollen cloak ; here he remains for twelve hours, and then, for the first time, he is rubbed over the whole body, near the stove, with the following ointment—

℞ Sulphuris Depurati ℥i.

Pulv. Radicis Helebori albi ℥ii.

Kali Nitrici gr. x.

Saponis nigri ℥i.

Adipis suilli ℥iij.

M. ut fiat unguentum.

After having rubbed in this ointment, the patient lays himself similarly wrapped up as before in his bed, and after twelve hours more a second rubbing in is performed, and again after twelve hours a third and last rubbing in is accomplished. After this, having lain for another twelve hours, he is put into a warm bath, in which every trace of the ointment is carefully removed, by rubbing him with black soap and woollen cloths.

The patient, now cured, is provided with clean linen and purified clothes, and taken to another room. In summer he may be permitted to depart after an interval of forty-eight hours, being now perfectly free from itch ; in winter it would be prudent to prepare him, by keeping him in a moderately warm room for two days, before allowing him to go into the cold air.

It is clear that this method of curing scabies in a short time, and without injury to the health, howsoever simple it may be, is yet attended with great difficulty in its application in certain private houses : but in all large cities where the number of operatives affected with this disease is great, and where quickness of cure at a small cost, renders an itch institution a desirable object, very little difficulty will be found.

But since for some years, in many districts, the itch has been observed to spread very generally, and by an imperfect treatment of some cases only, constantly continues, so in such districts the instituting of a proper place with baths and rooms easily warmed, as well as procuring other slight matters which might be necessary, could not meet with much opposition, and we should hope that from its striking propriety, that wheresoever the extension of the disease renders it necessary, such institutions may be formed and put into action.

It were desirable that such arrangements, as well as the conducting of the curative process, should be undertaken alone by persons perfectly understanding them : we would remark also, that without perfect cleansing of the beds, linen, and clothes used by each patient, after he is cured, and above all a regard for cleanliness, a new infection is always to be feared. A slight tax on those itchy disposed families who make use of the institution, would very soon cover the expenses.

Institutions of this kind in the cities, at a very small expense, might give opportunity of treatment to itch patients in the neighbouring districts.

The institution here requires for undertaking the cure of a patient placed therein, two Reichsthaler, (6s. 6d. British;) for this he has diet, baths, medicine, and medical treatment.

Of the persons affected with scabies in this institution, twelve individuals, cured in the manner described in two and four days, were detained a much longer time for medical inspection, all remained well, and not one suffered a relapse.

Treatment of Scabies by simple mechanical Means, from Berlin Medical Times.—The administration for hospital affairs has caused the treatment recommended by Staff-Surgeon Dr. Köhler to be employed from June to the end of December, on all the patients affected with scabies in the Charité. This treatment is purely mechanical, and consists simply in frictions with a mixture of finely powdered brick dust, (Ziegelmehl.)

The result of this treatment has been, that in the time mentioned 578 patients have been discharged from the institution, who have remained there a period of 10,576 days, which allows eighteen days and a half on an average for the cure of each patient.

Now as in the ordinary treatment pursued at an earlier period in the Charité, by means of frictions with an ointment composed of green soap and sulphur ointment, on the average fourteen days were necessary for cure, and as it has occurred frequently in the seven months before specified, that persons who appeared perfectly well when discharged, have again returned affected with itch, which has not been remarked as of such frequent occurrence during the period when the sulphur ointment was employed; therefore, for the interest of the patients, the hospital conductors have thought fit to abstain from the further use of mere mechanical means; and again to return to the more successful method, that of rubbing in the sulphur ointment before mentioned.

Union of the Scalp by the first Intention, when nearly the whole of it had been detached. (Communicated by Dr. Brach in Neustadt.)—In July of this year, (1836,) a young and robust slater 19 years of age, fell from a roof in Neustadt, on account of a scaffold which he was fastening giving way: he struck first with his head upon the roof, and slid along the tiles, and then fell with his body turned sideways upon old building wood and rafters upon the ground. There he lay without motion, and was thence borne to his dwelling, which was near.

I arrived about a quarter of an hour after the accident. The wounded man had during this time come to himself again; he sighed and whimpered, and breathed tolerably tranquil: his pulse was small and frequent, and at times irregular; his consciousness was very much disturbed, he spoke nonsense, and either answered wrongly the questions put to him, or did not answer at all. His countenance was pale, and from time to time vomiting occurred.

There were no contusions on his body, except, upon his left side, on the hip and arm on which he had fallen; his head on the contrary presented a wound of immense circumference; the whole scalp was severed from the middle of the frontal bone, right and left, to both ears, and then torn loose in its whole extent as far backwards as the occipital protuberance, so that the whole scalp could be turned back, almost like a night cap; very nearly the whole of the skull was laid bare, and thus looked like the skull of a subject, prepared for the removal of the brain by a circular incision with the saw. Besides this, there was a long tear in the middle of the scalp, which passed nearly to the vertex. It was also somewhat torn on the sides. In many places the pericranium was shaved off with the scalp nearly a quarter of an inch square, but there was no discoverable injury done to the bones of the skull. When I drew the scalp forward again, it adapted itself in all points to its old situation, and remained very well placed, as we may say, of itself.

I now began to clear the immense wound of hair, fragments of slate, and such like substances, of which a great quantity was concealed in the wound, and which was truly a laborious task, and with the shaving of the head lasted a long time. We were obliged often to give restoratives to the wounded man, on account of the frequent vomitings and faintings with which he was affected. After the requisite cleansing of the wound had been effected, I laid the flaps again *in situ*, united them with long straps of sticking-plaster, laid a simple compress upon it, and secured the whole by means of a cloth brought behind and under the chin. Internally, I gave him an infusion of arnica, with sulphate of soda.

The bloody suture seemed to me unnecessary, as every thing appeared to lie as neatly as possible with the dry suture, compress, and bandage. However I considered that in so considerable a wound of the head, as soon as the present symptoms of concussion had passed, there certainly would be an inflammatory reaction of the brain, and that then the patient, in a fit of delirium, might tear off all the bandages; at the same time I thought that the bloody suture would not be able to prevent this misfortune, wherefore I remained satisfied with the dry suture. Cold applications I did not think indicated either, because they would loosen the bandages, and on their firmness all chance of this immense flap becoming united by the first intention, depended. How could a remedy for such exposure of the whole skull be expected without union of the flap? If the flap did not heal what would be the consequence of such an exposure of the whole skull? and how could this immense mass of skin be replaced? On this account I confined myself to the simple means I have mentioned, endeavoured to keep the patient in the greatest quietude, caused him to be carefully watched, the chamber to be darkened, all excitement and disturbance to be avoided, and had determined on the first trace of inflammatory reaction setting in, to meet it by venesection, and internal remedies. These symptoms shewed themselves the next day towards the afternoon. The patient

became uneasy; the face, lately pale as that of a corpse, became flushed; he complained of a burning sensation over the whole head, was feverishly hot in the entire body, and again raved more distractedly than he had done in the forenoon, although from time to time during the night he had slept tolerably; the vomiting also, which had ceased towards morning, began again. The tongue was coated with yellow fur; the patient insisted on getting out of bed, and was detained there with the greatest difficulty; his pulse was tolerably full, strong, and hard. The bandage yet sat very well, on which account no change was made. Rest and sleep followed in an hour after venesection. Two grains of calomel were now administered in place of the arnica and sulphate of soda, which caused several watery stools during the night.

The next morning the patient was remarkably tranquil, and a considerable remission of fever had occurred, yet from time to time there was the same distracted raving; he had no recollection of his fall from the roof. In the afternoon a new exacerbation of the fever came on, with great restlessness, and increased delirium. After again bleeding him to nearly twelve ounces, he became again much more tranquil.

Matters went on the same way on the third day, when a pound of blood was abstracted. Internally he was given tartrate of kali dissolved in chamomile tea, with a strong addition of watery tincture of rhubarb.

On the fifth day, towards morning, a warm perspiration came out over the whole body, after which the heat and restlessness entirely left him, and the pulse lost its abnormal hardness and frequency. The vomiting had entirely ceased two days before, and the tongue had cleaned in great part. The bandage sat perfectly well; the sticking plaster still adhered firmly, and the whole scalp was applied accurately to the skull. The great anterior gash in the skin, which passed from the brow on both sides to the ears, suppurated in some points; pressure upon the head gave some pain, but not much, and the sensation of burning in the head had entirely ceased.

Similar internal treatment was still pursued. On the sixth day he was able to get out of bed without assistance; his tongue was cleaner; appetite again natural; delirium had ceased; he was very forgetful, and found difficulty in expressing his thoughts.

On the eighth day from the accident I removed the bandages entirely, and this whole, immense flap was found to be perfectly united in the identically same situation to the pericranium from which it had been detached, and it was alone the anterior wound from the forehead over the temples, which suppurated in some places, but in the third week it had perfectly healed in all points, and recovered its colour.

Forgetfulness and wandering of mind lasted still four weeks. In the fifth week the invalid, despite of my prohibition, again ventured upon the roof.

Nine weeks after this accident he fell again along with another

slater from a roof, on account of a scaffold again breaking loose ; the other broke his arm ; he fell more luckily, and only received a slight contusion on the leg ; after this he remained unalterably constant in his passion for slating.

Communication of Animal Poisons to Man.—A strong peasant at C——, stuck the fore-finger of the right hand, which was free from any injuries, into the throat of a goose which had dropped down suddenly, (which goose unfortunately was not examined accurately,) in order to convince herself if any foreign body might be contained therein, which was choking the animal.

Soon after this, in other respects fruitless examination, this woman felt violent darting pains in the finger, which soon extended over the whole hand ; very great swelling also soon occurred. Surgeon Kalusky of Cronstadt, two days after this occurrence, found the hand and the whole of the right arm considerably swollen ; the swelling was doughy to the feel ; the index finger was of an enormous size, and the skin of a bluish red, and from it very violent and irritating pangs extended to the arm-pit. Her general health was at the same time much affected ; she suffered from weariness, vertigo, and faintings. The surgeon compared the state of the patient with that of those persons in hot climates who have been bitten by the viper. Having internally administered musk, and employed externally the compound spirit of angelica, the woman got perfectly well in a comparatively short time.

To this occurrence I would add as a supplement :—

The Communication of an Herpetic Poison from Calves, which I have perceived to be no unfrequent occurrence now that my mind was directed to the subject ; and I have seen, since having communicated the above, again three persons infected after this manner. From the cases which have become known to me hitherto, either by examinations after death, or through the communications of other observers, I think I may conclude, that—

1st. As in the rot affecting highbred and common sheep, so that the mange is of a much more inveterate nature in high bred calves than in the calves of our common cattle. That

2nd. The communication of this mange poison can occur both from the common breed of calves of the country, and from those brought from abroad ; and

3rd. That the mange communicated to men from high bred calves is more inveterate and difficult of cure, than that communicated by the young of the common cattle of the county.—*Meyer in Creutzburg.*

Dr. Kolley has remarked, at Preisewitz, that male and female servants who have had the care of horses and horned cattle, affected with mange, have been attacked with a similar eruption on the arms, face, and breast, combined with remarkable constitutional effects. The use of a solution of the chloride of lime cured the eruption both in men and beasts in the space of four weeks.

Of Antimonial Suppositories as a Mean of restoring the Hæmorrhoidal Flux.—The physicians of past ages, have, perhaps, too much exaggerated the importance of hæmorrhoids in the scale of pathological phenomena, while those of our own time are fallen into the contrary extreme.

It cannot be denied that the suppression of the hæmorrhoidal flux, when habitual, may be productive of general disorders among men, almost as serious as the suppression of the menses in women. Moreover, it is as generally admitted, that with certain persons who have, not only regularly, but at indeterminate periods, a draining or hæmorrhoidal flux, the existence of this pathological condition is attended with a state of general good health; although it may remain for a long time uncertain and variable, provided the hæmorrhoids do not manifest themselves as soon as usual. Observation shows also, that persons who have had hæmorrhoids for a long time, suffer generally, if this flux entirely ceases. And it often happens that there is a call for its restoration.

Many means have been advised to effect this indication. The warm local baths, mustard foot baths, leeches to the part, suction applied to the lower part of the large intestines, purgatives, and cupping glasses to the part. Of all the means which we have made use of, only one has succeeded in any satisfactory manner. This is the application of cupping glasses. This mean was entirely forgotten, when a student of the Medical Faculty of Paris restored it to honour, and I am able to bear witness to its effects on him.

He had had hæmorrhoids till the age of twenty years, and always enjoyed good health. This flux now ceased, when he became subject to violent pains in the stomach, and continual disorders of the digestive organs. He consulted M. Andral, while attending the Hospital de la Pitié, and this physician made use of every mean advised by authors for restoring this flux. Nothing succeeded, and the disease remained stationary. The young patient then conceived the idea of applying a cupping glass to the part. During this application the circumference of the anus enveloped the hæmorrhoidal tumours, which for eight days were swollen and painful. From this time his health was re-established. A month after this he experienced a slight return of gastric disorder; and one day, while attending my visit to the hospital, he spoke to me of the relief which he had obtained the previous month from the sufferings which he now began to feel again, and offered to let me witness the prompt appearance of the hæmorrhoids under the operation of the cupping glass. I accepted the invitation with alacrity, and at the same time I placed him upon the bed of one of the patients, and in the presence of more than forty physicians and students, I applied a cupping glass to the fundament. A minute did not elapse when the tumours made their appearance, and becoming united, they acquired the size of a small pigeon's egg ten minutes after the application of the instrument. The same means were made use of the following day, and the hæmorrhoidal flux continued for a week, and was followed by a cessation of the disorders of the stomach.

M. Andral also saw this young physician, and can testify with me to the great rapidity with which the tumours became swollen.

After this I had only one opportunity of locally applying cupping glasses for recalling hæmorrhoidal flux. This was with a female afflicted with erratic rheumatism, which to me appeared to be caused by the suppression of habitual hæmorrhoidal flux. I succeeded in puffing up the hæmorrhoidal vessels by means of the cupping glass; but the tumours disappeared soon after the application of the instrument. What prevents my using this remedy more frequently is this: in the first place, patients, especially women, have a great aversion to it; secondly, I have conceived that a much more simple remedy, and the employment of which can never be the subject of serious objection, will answer the same end, I allude to antimonial suppositories.

As I had never succeeded with aloetic suppositories, I thought by substituting in the place of aloes one of the most energetic irritants I might attain the desired end. Now, tartrate of antimony, applied locally to the skin or mucous membrane, creates an inflammatory action very powerful and persisting, I therefore preferred this article. I mix with a drachm of butter or lard, from two to six grains of tartrate of antimony. The suppository, being introduced within the sphincter of the anus, melts quickly, and the tartrate of antimony remaining in contact with the mucous membrane, excites a lively local irritation, a species of tenesmus, as a necessary consequence. When the suppository contains only a grain, or half a grain of the tartar, it can be retained for twelve hours without the necessity of going to stool; but when a greater quantity of it is made use of, the patient experiences a heat, at first slight, but afterwards scorching, and attended with painful pulsations at the part; there is a necessity of frequently going to stool. The arterial pulsations increase at the same time that the circumference of the anus protrudes, and pustules, similar to those excited by tartar emetic on the skin, now appear; bluish tumours arise, hard and painful, permitting occasionally a large quantity of blood to transude. These are the true hæmorrhoidal tumours, perfectly evident with those who have had them already, and only apparent with those who have not had them.

I have often advised this remedy, in consultation, in my private practice, and in the hospital. I have taken notes of only six patients who have used it. With five of these the hæmorrhoidal flux was re-established; in the sixth, who had never had hæmorrhoidal tumours, I was unable to excite them. I will briefly relate these six cases, as they afford some practical interest.

CASE I.—A man 36 years of age, had an hæmorrhoidal flux until his thirty-fourth year. The flux appeared at irregular periods four or five times a year. At each time it lasted at least five days. He took a cold which at first seemed to be of little importance; but during the continuance of the cold, hæmoptysis came on, and the hæmorrhoids disappeared. The cold still continued, and the hæmoptysis was repeated, and lasted for several days without any means being able to

arrested it. Soon after this symptoms of phthisis pulmonalis were manifested, and he concluded to enter the Hospital Dieu.

There was considerable emaciation, and slight hectic fever; resonance less at the upper part of the right lung, both before and behind; murmur in inspiration scarcely perceptible—in expiration it was very strong; slight broncophony; rale sub-crepitant, sufficiently frequent; some humid crackling and a trace of pectoriloquy; expectoration muco-puriform, and often streaked with blood; but little appetite. These were the symptoms of tubercles beginning to be formed in the lung. Nevertheless, having regard to the suppression of the hæmorrhoids, I conceived that there might exist in the respiratory organs a point to which there was an undue flow of blood, similar to that which lately existed in the pelvis. I determined on recalling the hæmorrhoids. I made an application of a suppository of tart. antim. It excited a glow of heat around the anus, and the following night, large hæmorrhoidal tumours appeared, which continued six days, and discharged abundantly. A great amelioration of the thoracic symptoms took place; the expectoration immediately ceased to be bloody, and was much diminished, so that five days afterwards, he coughed but little, and respired almost as strong as usual. Soon after this the piles appeared without solicitation, and the threatening symptoms which had been manifested in the breast were completely dissipated. Indeed, I do not imagine that I cured phthisis pulmonalis, but merely an obstinate congestion of the lung, which probably would have ended in inducing tubercles.

CASE II.—A cook, aged 50 years, entered the Hospital Dieu, in order to be treated for a chronic disease of the stomach. This man stated, that three years previous to his entering the hospital, he had been afflicted with piles, which returned every month, and which at that time became suppressed. Since then he had lost his appetite, the little which he did eat gave rise to pains in his stomach, attended with obstinate constipation.

He had made use of local bloodlettings, opiates, laxatives, magnesia, &c., in vain. I thought that the hæmorrhoidal flux ought first to be re-established if possible. For this purpose I applied a suppository containing the tart. antim. The first suppository gave rise to a diarrhœa and tenesmus, but no tumour. The next day another suppository, containing three grains of the tartrate antimony to the drachm, (a little stronger than the previous one,) was made use of. This caused a very sharp pain at the anus, and on the third day we found that very large and painful hæmorrhoidal tumours had arisen. These tumours remained swollen for several days, without applying any suppository, but did not discharge. His health was improved, but was not completely established. A month after this, a new application of the suppository caused the piles to discharge, and at the same time his stools became bloody. This discharge continued for six days and then ceased. The health of the patient became much improved, and after using the waters of Vichy for three weeks, his cure became confirmed. I have not since seen the two patients whose

cases I have just related, so that I am unable to say what their present state of health is.

CASE III.—A woman, aged 40 years, was attacked with obstinate erratic rheumatism. I imagined that by provoking hæmorrhoids, she might obtain some relief, I therefore applied two antimonial suppositories. The tumours appeared; continued two days, and then disappeared; but the rheumatism was not mitigated. This case, apparently of no consequence, proves that hæmorrhoids can be excited in those who have never had them.

CASE IV.—A young man, aged 30 years, contracted a gonorrhœa, which was treated and cured with emollients and balsams. Soon after this he experienced all the symptoms of syphilis. The velum palati was destroyed with a large ulcer; the nasal fossa, and even the larynx, were not exempt from alteration. The prot. iodide of mercury, bathing with a solution of corrosive sublimate, and local cauterization with nitrate of silver improved the character of these disorders. As the patient had formerly had bleeding piles, which returned many times yearly, but had disappeared during the last year, I applied for two days in succession the antimonial suppositories. The piles appeared again and discharged abundantly; but there was no abatement of the disease.

CASE V.—In September, 1835, while attending the hospital, a patient, aged 45 years, came under my care. He had a very serious attack of sub-acute hepatitis. Hepatic enlargement was very distinct, and there also existed an effusion of the peritoneum and the cellular tissue of the pelvic organs. As he had a high fever, I took blood from the arm, but without any relief. I also applied leeches to the right hypocondrium, and over the whole seat of disease, but without any mitigation of the malady. The patient had had in the course of his life three or four attacks of piles. Knowing how much importance modern practitioners attach to hæmorrhoids, especially in diseases of the liver, I determined on exciting in the pelvic vessels a derivation which might be salutary. The application of an antimonial suppository produced on the second day very painful hæmorrhoidal tumours, which discharged much blood. Nevertheless, the disease increased in a frightful manner and the patient died.

CASE VI.—M. R., aged 52 years, during his own life had been subject to bleedings from the nose which returned many times during the month, particularly in the spring. His father, who died at eighty-two years of age, was subject during his life to hæmorrhoids, which flowed regularly every month.

The epistaxis had now been suppressed for three years, during which time he was subject to affections of the head two or three times a year, especially in the spring. He also had a species of cerebral excitation similar to that which marks the commencement of drunkenness. The application of leeches to the part moderated these symptoms considerably, but they soon returned and gave much uneasiness. The regularity of the hæmorrhoids in the father caused me to think that a monthly congestion in the rectum would be of great service.

During one year, M. R. applied for three days every month an antimonial suppository. This application excited a violent irritation, and an eruption which evidently did not differ from the ordinary eruption produced by antimony. The inflammation lasted only a few days.

Although hæmorrhoids, properly so called, did not supervene, the cerebral affections have not given any uneasiness, and his whole health would have continued to improve, if the patient had patience to continue the use of the means.

Let us recapitulate the facts contained in this brief account. Antimonial suppositories in six cases, in order to provoke hæmorrhoids. In five the hæmorrhoids appeared two days after the employment of the means. Four were truly hæmorrhoidal; one was not. In the four first, the eruption was persisting; in the other it lasted only two days. In one of the patients it was impossible to produce the tumours. Of the six patients, three were cured, probably in consequence of the appearance of the flux. One of these three might have been cured, although the hæmorrhoidal tumours, properly so called, were not produced. Three experienced no alleviation, although the hæmorrhoids were reproduced easily and abundantly. It would be too hasty to draw general conclusions from so small a number of cases, I wish only to make known to the faculty a therapeutic mean, by which they can fulfil indications which may sometimes be presented to their notice.—*Journal de Connaissances Medico-Chirurgicales.*

On the Uterine Syphon, &c., by Professor Montain of Lyons.—It not unfrequently occurs that accouchement becomes difficult from the rigidity and dryness of the foetal head and uterine passages, consequent either on the too early escape of the amniotic fluid, or on a state of irritation, such as to deprive this fluid of its usual lubricating properties. The pains then diminish, the parts become swollen and inflamed, and delivery is distressingly procrastinated. Having observed a considerable number of such cases both in my service at the *Charité* of Lyons, and in private practice, I have resorted to the following expedient, with uniform success. The operation is perfectly simple, and this is perhaps the reason it has hitherto escaped the attention of practitioners; but its simplicity cannot diminish its value, since its usefulness is fully established.

The instrument is a silver canula, from five to six inches long, slightly curved, terminated by a flattened oval extremity, very thin, and perforated on both surfaces by a great number of small holes; the other extremity is constructed so as to receive the end of the canula of an ordinary syringe.

This instrument, which I denominate an *uterine syphon*, is well oiled, and directed so as to introduce the flattened extremity between the foetal head and the uterus, which may be done without difficulty and without pain, and, with a syringe, tepid olive oil is injected so as to anoint the head and passages. The canula is successively carried around the head, the passages soon become soft and yielding, and the head escapes without difficulty.

I might relate a great number of cases on this subject, for the accouchements susceptible of relief by the *syphon* are of frequent occurrence. I will at present give but two.

CASE I.—Mrs. L. had been in labour more than twenty-four hours, when I was requested in consultation with her physician. The pains were severe, but the waters had long since escaped. I proposed injections by means of the *syphon*, but yielded to the desire of the attending physician, who preferred the forceps. The application of these, however, was attempted in vain; injections were used, and the head immediately passed out.

CASE II.—Mrs. C. had strong pains, and was extremely nervous; the waters had passed off three days before; the parts were dry and apparently inflamed; according to the midwife, the pains had been strong for several hours, but had effected no change in the position of the child. Injections were made around the child's head, and it almost immediately came through with the greatest ease.—*Gazette Medicale*.

M. Montain also recommends the injection, by means of the *syphon*, of a tincture of ergot into the uterus, as preferable in many instances to its administration by the stomach. The preparation he uses is made by digesting half an ounce of ergot in three or four ounces of alcohol, and keeping the bottle well stopped. Of this, one or two table-spoonfuls may be mixed with tepid water, and thrown into the neck of the uterus; to be repeated if necessary. If the action be too powerful, it can be moderated by sedative, and, subsequently, by demulcent injections. M. Montain relates two cases in which this medication was signally successful.

On the Typhous Fever, which occurred at Philadelphia in the Spring and Summer of 1836; illustrated by Clinical Observations at the Philadelphia Hospital; showing the Distinction between this Form of Disease and Dothineritis, or the Typhoid Fever, with Alteration of the Follicles of the small Intestine.—By W.W. GERHARD, M. D., one of the Physicians to the Hospital. [We have thought it right to make copious extracts from a paper, published by Doctor Gerhard, on Typhus, in the American Journal of Medical Sciences last February. We have omitted all the cases and dissections detailed in the original paper, and which furnish ample evidence that Doctor Gerhard has arrived at his conclusion by means of an induction sufficiently extensive and accurate; and we rejoice to find that his inquiries have been materially guided by papers originally published in this Journal.]

During a residence of two or three years at Paris, I had studied with great care the pathology and treatment of the disease usually termed, in the French Hospitals, typhoid fever, or typhoid affection. There is another designation for it, founded on its anatomical characters, and therefore more directly in accordance with modern medical nomenclature; it is dothineritis. This variety of fever, which is identical with the disease termed typhus mitior or nervous fever, is frequent at Paris, and is almost the only fever which can be said to be endemic there. Intermittent and remittent fevers are rarely seen

except amongst those individuals who had already contracted some form of these diseases in the malarious districts of France. Some slight fevers, attended with a whitish or yellow tongue, and gastric symptoms, occasionally occur; they scarcely assume the form of a fixed disease, and usually disappear under a very simple treatment.

These fevers were the only ones known at Paris for some years past; but in 1813-14, there occurred a severe epidemic fever, characterized by extreme prostration, and strongly marked cerebral symptoms. This epidemic was first noticed amongst the troops who returned from Napoleon's unsuccessful campaigns in Germany and the east of France; it afterwards spread amongst the inhabitants of Paris and other large cities, and was every where extremely fatal. No accurate description of this fever is on record, although it was witnessed by several of the most distinguished French physicians. Some of these, more especially Louis and Chomel, are inclined to consider it as identical with the prevailing dothineritis, but their opinion is probably erroneous, and the disease, as far as we know, should be classed among the forms of continued fever, distinguished by the terms typhus, typhus gravior, petechial or spotted fever, &c.

There are, however, complete histories of the typhoid fever or typhoid affection, or dothineritis, (all names belonging to one disease.) It is one of the most frequent and the most severe acute affections observed at Paris, and has been studied with extreme accuracy, more especially by Louis and Chomel, who have both published admirable descriptions of it. The work of Dr. Louis is especially interesting and is a model in its kind; he has analyzed the symptoms and pathological phenomena of the fever so accurately and fully, as to surpass any other description of individual diseases. The typhoid fever was placed by this work of Dr. Louis, in the same relation to other fevers that pneumonia holds in reference to the affections of the chest. They are both so well studied, and their symptoms are so well known, that they serve as types with which other less thoroughly understood affections may be compared.

It affords us, then, great advantages in the investigation of the history of fevers, to begin with the typhoid, as the best known of these affections. Assuming this disease as the basis of our investigations, one great point is gained, and much greater certainty can be given to our ulterior researches, if we compare the symptoms of any fever which is little known and imperfectly described, with those of the typhoid fever, or dothineritis, as it is now frequently called from its anatomical lesion.

This inquiry was in accordance with a desire which I had long cherished, of investigating the most common fevers in the middle states of America, where, from our geographical position, we witness the fevers observed at the northern, and occasionally those of the southern states. The commercial relations of Philadelphia are so frequent with the whole southern coast of the United States, and the passage to the north so rapid in the summer and autumnal months, that we receive into our hospitals a considerable number of patients taken ill on the

coast of North Carolina, Virginia, and even Alabama and Louisiana. There are, therefore, few places where such a study could be pursued to more advantage than at Philadelphia. During the last three years of a constant connexion with our largest hospitals, either as resident or attending physician, I have not lost sight of this object of study, and I have already published in the *American Journal* for the year 1835, some cases of the dothineritis as well as the remittent and intermittent fevers.

Dothineritis is by no means a rare disease at Philadelphia, although less common than at Paris. In the essay alluded to I established the identity of the anatomical characters and of the symptoms of the fever occurring at Philadelphia, with that observed at Paris. I also showed that the patients were chiefly those who had resided but a short time at Philadelphia, and that they were taken ill on ship-board, or under some other circumstances causing an abrupt change of food and habits of life. They were also young persons, but few having passed the age of twenty-five years. Both these conditions of age and change of habit are to be observed to be essential to the development of typhoid fever at Paris.

Having once established the complete identity of a fever which is so common at Paris and so well described, with a similar affection not unfrequently met with at Philadelphia, I examined the pathological phenomena of our remittent and intermittent fevers of the severe malignant character so frequently observed along the southern coast, and sometimes occurring in those malarious parts of the country which are situated within a short distance of Philadelphia. In all these fevers the glands of Peyer, as well as the other intestinal follicles, were found perfectly healthy; the large intestine was occasionally but not constantly diseased, while the stomach, and to a still greater degree the liver and spleen were invariably found in a morbid condition. If the fever proved fatal in the course of the first fortnight, the liver and spleen were softened as well as enlarged; but if the disease assumed a more chronic form, the viscera were hardened as well as hypertrophied. The latter state was the first stage of these chronic lesions which are formed in the livers of patients long affected with remittents or intermittents, and which continue throughout the course of the ascites, which is so common a consequence of these diseases. I made numerous examinations of the bodies of patients who died of the same variety of malignant remittent and intermittent during the summer of 1835, and still more frequently in the epidemic of 1836, a year in which these diseases have been unusually fatal throughout the southern states. The results of these late examinations have confirmed those already obtained, and showed that the follicles of the small intestine are free from lesions, and that the anatomical character of the disease is to be looked for in the spleen, liver, and stomach.

The bilious and yellow fevers are probably referrible to the same class as the malignant remittents, but in yellow fever the disorganization seems to be most extensive in the stomach, whence arise the black vomit, which forms a characteristic symptom of the

disease. Bilious fever, or, in other words, the remittent fever attended with unusual alteration of the liver and a disordered secretion of bile, is common with us. Yellow fever is rare, and occurs in an epidemic form at such long intervals, that I have seen but few cases of it.

The typhous fever, which is so common throughout the British dominions, especially in Ireland, is not attended with ulceration or other lesion of the glands of Peyer.* From the account of the lesions presented by most of the writers upon the subject, it would seem that there is no constant anatomical lesion, but that the lungs present traces of disease more frequently than any other organ. My own observation of this variety of fever was limited to the examination of the fever patients under the care of the late Dr. Gregory of the Edinburgh infirmary. This observation was not sufficiently long or accurate to enable me to do more than refer to those physicians who have enjoyed extended facilities for the study of this affection. The lesion of the glands of Peyer is now well known to the British physicians, but an error frequently committed by them is that they regard this affection (dothineritis) as a mere complication of their ordinary typhus, or a modified form of it. At least I do not at this moment recollect any one who has clearly stated that the two diseases are always distinct, before the publication of a note in the *Dublin Journal*, by Dr. Lombard of Geneva, (Sept. 1836.)

It is not possible to set this matter at rest, unless a series of accurate histories of typhus with detailed symptoms and pathological lesions should be published by British physicians. With the aid of a statement of this kind, such a comparison might be made as to set the points of difference between the ordinary British or Irish typhus and the dothineritis of France in their true light. From the information we possess, we should conjecture that the two diseases are widely and entirely different in symptoms, anatomical characters, treatment, and mode of transmission. But the British typhus seems to us to be identical with the disease which forms the subject of the present memoir, and is apparently the same affection which is variously designated typhus gravior, ship fever, jail fever, camp fever; sometimes petechial or spotted fever. The term typhus mitior of the older writers seems nearly synonymous with that of typhoid fever or dothineritis of the French Physicians.

In America there have occurred several epidemics of fever more or less similar in their nature to the British typhus. Some of these were confined to the New England States, where they were often known under the name of spotted fever, and are described by North, Hale, and others. Other epidemic diseases of a similar type extended to a larger district of country, and overran a considerable portion of the Middle States, causing extensive ravages both in town and

* I mean that this lesion, when it occurs, is merely accidental, or a complication not occurring in the ordinary course of the disease.

country. It was of epidemics of this kind that many distinguished physicians of Philadelphia perished in different years, amongst them were the professors of the University, Rush, Wistar, and Dorsey. No distinct history of the typhous fevers which prevailed at Philadelphia at different periods between the years 1812 and 1820 is on record. I mean such an account of the disease as makes its diagnosis so clear that there can be no danger of confounding it with other analogous affections. The fever was well studied by the physicians who practised at that time, but the habit of analyzing symptoms had not been introduced, and their experience, however valuable to themselves, was in a great degree lost for their successors. These remarks are so true, that although an eminent physician of Philadelphia pronounced the epidemic of 1836 to be the same as that of 1812 and succeeding years, another distinguished medical gentleman who was not familiar by his own experience with the former disease, considered them as distinct affections, and that the one which first occurred was a low grade of pulmonary inflammation.

That the fevers were really identical, was proven by the opinion of Dr. Parrish, one of the most experienced physicians of Philadelphia, who practised very extensively amongst all classes of inhabitants in the winter of 1812-13, and was remarkably successful in his treatment of the prevailing fever. He saw some of the cases at the Philadelphia Hospital in 1836, before the disease had extended to the wealthier classes, and immediately recognized its true character.

For a period of at least ten years there has been no epidemic of this nature at Philadelphia. In the year 1827, a large number of Irish emigrants were ill of a typhoid fever, with ulceration of the small intestines, which was probably dothinerteritis, and during several successive years there were more or less extensive epidemics of remittent and intermittent fevers, occurring in the neighbourhood of the city, but not often extending into the central parts of the town. Occasionally, sporadic cases of fever of a comatose or typhoid character would occur, but these cases were nearly always either some form of malignant remittent, or else they occurred during the winter months, and were complicated with pneumonia. The inflammation of the lungs then appeared as the first stage in the disease, which afterwards assumed those cerebral symptoms of stupor and feebleness, which have procured for it the designation of pneumonia typhoides. These cases I often witnessed while resident physician of the Alms-house Infirmary during the years 1828-30.

At Boston, in the year 1833, there was an epidemic dothinerteritis of extreme gravity, and unusually fatal. This fever was well studied by the late James Jackson, jun., and other physicians, and was proven by them to be identical in symptoms and pathological lesions with the typhoid fever of Paris. Some of the physicians of that city are inclined to regard their former epidemics as of this

nature, but this opinion seems to us more than doubtful. Since the epidemic, the typhoid fever is there a common sporadic disease, rather more frequent apparently than at Philadelphia.

In the winter of 1835-36, there was an unusual number of cases of gangrene of the lungs at the Philadelphia Hospital, and but few of decided pneumonia. Several cases of dothinerteritis occurred in the autumn, but there were few afterwards. During the winter, a form of fever not commonly met with at the hospitals, was observed from time to time. It was characterized by pungent burning heat of the skin, dusky aspect of the countenance, subsultus, delirium, with great stupor and prostration; but there was no diarrhœa, and but few other symptoms referrible to the alimentary canal. It was the disease which afterwards appeared as an epidemic. At first it was not well understood by us, was sometimes confounded with bronchitis or pneumonia typhoides, from the complication of pulmonary disease with the symptoms of the fever. These cases recovered under the use of a mild stimulating and supporting treatment, with one exception, in which death ensued from sloughing of the sacrum and gangrene of the lungs.

In the early part of the month of March, the admissions for the fever were more numerous. They attracted the greater attention from their occurring in groups of several from the same house, and almost all coming from a particular neighbourhood. Amongst the very first admitted were seven negroes, the entire population of a cellar in the lower part of the city. The symptoms varied but little in the seven cases, and upon an examination of two of the number who died, no lesion of sufficient importance to account for the symptoms could be detected.

As soon as these patients were admitted I resolved to note with care the pathological lesions presented by the bodies of most of those who should die of the fever, examine its symptoms and ascertain the influence of therapeutic agents upon it. This research was commenced with a view to obtain more precise notions as to the character of an epidemic which has probably more than once appeared in America, and seems to be endemic in Great Britain and Ireland. It was especially desirable to ascertain if there was a real fundamental difference between the form of disease which prevailed this year and the dothinerteritis which is always to be met with in America, as a sporadic affection. My friend and colleague, Dr. Pennock, had charge of one-half the medical wards of the Philadelphia Hospital; his observations were conducted at the same time with my own, but the autopsies and the examination of doubtful cases were always made in the presence of both of us. Dr. Pennock noted a large number of cases, and has given me the privilege of adding his collections to my own. They are the more valuable from the familiar knowledge which he obtained of the dothinerteritis in the wards of La Pitié at Paris. Our inquiries were conducted so much in concert, and our opinions as to the symptoms and treatment of the fever were so often compared together, that this memoir is

in most respects the expression of the results obtained by our joint labours.

A portion of the cases were treated by Dr. Pancoast; of these I have no notes excepting such as were obtained from the registers of the wards; they were chiefly admitted towards the close of the epidemic season, when we had already procured a large mass of materials.*

Many of the observations are deficient in the history of the early symptoms, as the patients at their entrance into the hospital often did not retain intelligence enough to recollect the previous symptoms of their disease. The autopsies were always made with great care, more particularly the examination of the small intestines; but the weight of occupation and the ennui of recording results which varied so little among themselves, caused us to neglect committing some of them to paper. We have, however, noted in detail a very large number, showing the nature of the lesions; and we always took great care to remark the diseased or healthy state of the organs. We are quite sure that nothing of importance escaped us, and, above all, that the condition of the follicles of the small intestine was carefully ascertained. Our mass of facts is so considerable, that many important questions will be solved by them in relation to the history of this form of continued fever. They will clear up many questions relative to the disease; for although few cases are as complete as we could have desired, the information which is wanting in one case may be gathered from others; none are deficient in all the particulars, or fail to give a tolerably exact statement of the symptoms at one period or other of the disease.

In our investigations, we availed ourselves of the opportunities we possessed to inquire into the pathological anatomy, the symptoms, the mode of communication, and the treatment of this fever, which had not been witnessed at Philadelphia for some years, even if it were the same disease as that of former epidemics. At each step of this progress, I shall compare the facts before us with those relating to the history of the typhoid fever, or dothinerteritis, and when the symptoms differ, it will be easy to draw the line of distinction between two diseases, differing in their treatment, symptoms, duration, and pathological lesions.

There is some confusion in the designation of these fevers, but it is not my intention to enter upon a discussion of their nomenclature. It is sufficient to state that in using the terms typhus, typhous fever, typhus gravior, spotted or petechial fever, I mean that disease which

* These inquiries were greatly promoted by the zeal and industry of the resident physicians of the hospital, who were all much interested in the examination of the disease, and untiring in their efforts to relieve the suffering of patients who always required much more than ordinary care. In the rotation of service the most arduous duty fell to the lot of Drs. Bush, Stille, Patterson, Elmer, Frisby, and Johnson, of whom the two last mentioned were themselves attacked with fever.

forms the subject of this memoir, and that by the terms typhus mitior, typhoid fever, or dothineritis, I mean the disease described by Louis, Chomel, &c., and attended with a lesion of the glands of Peyer.

The number of cases admitted with typhus, was 214. Of this number there were 120 men and 94 women. A few cases who were at the same time in the wards, and already under treatment for other diseases, are not included, although they were afterwards affected with the prevailing epidemic, but their names on the register present only the disease for which they had been admitted. The whole number of cases is, therefore, from 230 to 250. A large majority of the 214 patients were negroes or mulattoes, there were 147 people of colour and 67 whites. The disease first appeared in the former class of patients, and always prevailed more extensively amongst them than the whites who were living in the same part of the town, and exposed nearly to the same causes of disease.

The patients were taken with the fever in various parts of the city and neighbouring districts, but by much the greatest number came from that part of the town which extends from Lombard-street to a little below Shippen, and from Fifth to Eighth streets; this small but crowded district became almost an infected suburb. Within these limits the poorest and most intemperate of the inhabitants of Philadelphia reside. It is the St. Giles or the Faubourg Saint Marcel of Philadelphia. The filthiest and most crowded alleys offered the greatest proportion of patients. Thus, Small-street and St. Mary's-street, with the numerous courts and alleys running from them, contained many more sick than other streets inhabited by a population nearly as poor and intemperate, but less crowded. The different streets were not infected at the same time, thus the earliest patients were taken ill in Shippen and in Small streets, while St. Mary's-street, which furnished an immense number of patients, was comparatively free from infection until a month afterwards. The disease appeared very soon in the prison (now taken down) in Arch-street, but as the inmates of the prison come in great part from the infected district, it is possible that the disease may have been introduced by those who were admitted while labouring under it. Towards the close of the epidemic, patients were admitted in considerable numbers from some of the streets in the Northern Liberties, and throughout its whole course there were scattering cases from different parts of the city, and a few from the country, where there were no others ill in the house from which the patient had come. But few cases, however, occurred in the central parts of the town, where the inhabitants are generally in easy circumstances, and comfortably fed and lodged.

Classes of Persons affected.—The first patients were almost exclusively from the poorest and most intemperate class of people, chiefly day-labourers. Such was the case with most of the blacks, especially the men, who were almost without exception in the habit of drinking freely of ardent spirits. The women were without fixed occupations, or were servants out of place. As the disease extended to the differ-

ent parts of the city, people of various occupations were affected, amongst them there was one respectable physician, who died of the fever. The extension of the disease to those in easy circumstances, was shown in the practice of several eminent physicians of Philadelphia; they had not seen a case until the fever had prevailed some months at the hospital, although they afterwards met with it in their private practice.

Mode of Propagation of the Disease.—The origin of the disease is as unknown as that of most epidemics; according to the general rule, it attacked those who were sunk in poverty and intemperance, and huddled together in confined apartments. It also appeared at different and remote points, some miles distant from the focus of infection, without the possibility of tracing any direct communication between those already attacked. There was, thus, a general cause, which extended its influence throughout the vicinity of Philadelphia. But, besides the epidemic cause, from which the greater number of cases seemed to arise, the fever was evidently propagated in a considerable proportion of patients by direct contagion. Those who entered at an early period of the epidemic came in groups together, some from the prison, whole families from the same room or the same house. About that time I made a careful inspection of the district as one of a committee of the Board of Health, and in some instances we found houses completely vacated, the tenants being either dead or at the hospitals. In other cases, the whole or a large proportion of the inhabitants of a room were ill. It was rare to meet with a severe case without seeing others in the same house.

The evidence of contagion at the Philadelphia Hospital was more direct and conclusive. Three of the principal nurses, and about a dozen assistant nurses, besides a number of patients ill with various diseases, were taken with the fever. The three principal nurses belonged, two to the wards for blacks, where there were the greatest number of fever patients, and the third to a ward for whites, where there were several cases. There was only one nurse of a ward in which many of the patients were collected, who escaped, but several of his assistants and patients were taken ill. Two of the resident physicians in attendance upon the same ward, where the patients were most numerous, were also severely ill with the fever. On the other hand, no nurse from the part of the hospital where there were but few or no typhous cases, suffered, and the number of patients taken ill in the surgical or lunatic wards was very small, not exceeding six in number. The wards in which fever patients were placed did not contain more than a third or a fourth of the population of the hospital, yet the number of cases originating in them after the first introduction of the disease was at least four times as great as in all the other parts of the building. The Alms-house and House of Employment, which are separated from the hospital by a space of at least forty feet at the nearest points, furnished five or six cases, probably not more than the same number of poor in any other part of the neighbourhood would have done.

The proportion of attendants upon the sick who suffered was in exact relation to the number of fever patients in the ward; thus in the wards for blacks, (both men and women,) and in the men's medical, No. 1, scarcely an assistant escaped. In the other medical wards a few were taken ill, and in the surgical and lunatic wards all the nurses escaped. The matter of the contagion, be it what it may, was generally mingled with the air, but sometimes seemed to be combined with the pungent hot sweat of the patients. In some cases the contagion was evidently direct from body to body. This was established by the evidence of a nurse and an assistant, both persons of intelligence, and, from their familiarity with the disease, quite free from fear. The nurse was shaving a man, who died in a few hours after his entrance, he inhaled his breath, which had a nauseous taste, and in an hour afterwards was taken with nausea, cephalalgia, and ringing of the ears. From that *moment* the attack of fever began, and assumed a severe character. The assistant was supporting another patient who died soon afterwards, he felt the pungent sweat upon his skin, and was taken immediately with the symptoms of typhus.* The wards in which the fever patients were placed, were large and well ventilated. We were at first disinclined to believe that the disease would prove contagious, but as soon as the fact was clearly proven, measures were taken to remove the patients not yet affected from most of these wards, and, if it had continued for a longer period, an efficient local quarantine would have been adopted. Dead bodies either did not communicate the contagion or its influence was easily counteracted by favourable circumstances. Both Dr. Pennock and myself, and several of the resident physicians, were engaged nearly every day during the most intense prevalence of the disease in making long and laborious anatomical investigations, without suffering from the fever.

It is very clearly proven that the typhoid fever, or dothinerteritis, is not contagious. Dr. Louis informed me that, in the course of his long experience of the disease, he had never seen a single case originating in an hospital. I have seen but one. The contrast between the fevers, in this respect, is obvious.

Age of the Patients.—After childhood, the age seemed to exercise but little influence upon the susceptibility to the disease. But children were rarely attacked by it. None of the children in the Asylum attached to the hospital, where there were about two hundred, were taken ill. Nor in the inspection which I made as a member of the Board of Health, of the houses in the infected district, did I discover many children who seemed to be labouring under the fever. After childhood, the age of patients seemed nearly without influence. Thus, of sixty-six whites, there were thirty-five below the age of 35 years, and thirty-one beyond that age, and, on adding the number of nurses

* Two other cases of assistant nurses also originated from similar contact, but as they were persons of less intelligence, I have refrained from relating their cases, as they offer less undoubted testimony.

and patients taken ill in the wards, we shall increase the number of persons older than 35 years, or passed the middle of life. The blacks give a greater proportion of young persons, although there were patients amongst them who were far advanced in years. But their comparative youth is easily to be accounted for by the large number of blacks engaged as labourers and inhabiting the infected part of the town, very few of them are old or middle aged men. Another reason is the habit of the blacks to state themselves younger than they really are, partly from ignorance of the value of numbers and of the precise year of their birth. It would, therefore, give incorrect results to include them in our estimate. The age of these patients differs much from that of those affected with the typhoid fever, or dothineritis, who are all younger, the disease almost never occurring above the age of 35 years; the average for Paris and Philadelphia is $22\frac{1}{2}$ and 22 years. (See Louis on Typhoid Fever, and American Journal, 1835.)

The sex seems to exert little influence on the liability to the disease. The numbers were 120 men and 94 women, which is about the relative proportion of our ordinary patients. This result differs but little from that observed in typhoid fever, where, *cæteris paribus*, men are perhaps a little more subject to it than women.

The change of life and habits from country to town was of no importance, our patients were nearly all resident for some years at Philadelphia, and some had been paupers for many years; their food and mode of living remaining unchanged during that period. The disease was not, as dothineritis, nearly confined to those persons who had recently removed from one place to another.

Use of Ardent Spirits.—The most perfect temperance did not prove a safeguard when exposed to the contagion, as was shown by the cases of two of the resident physicians and of many others. Still, as a large majority of our patients were known to be intemperate, it would at first sight appear that intemperance was a powerful predisposing cause. But the habits of the day-labourers are such that but few of them abstain from using spirits more or less freely, so that the number of typhous patients who were intemperate does not greatly differ from that of those affected with other acute diseases. Most of the women were not given to intoxication.

Season of the Year.—The epidemic began in March, and continued until August. There were a few scattering cases afterwards. The summer was unusually cool, and the spring and winter cold. It was remarked, that as the summer advanced, and an epidemic dysentery appeared, the fever was changed in character, and frequently offered a new symptom, that is diarrhœa, which was wanting in the earlier months.

Occupation, &c.—Our tables do not give us all the necessary information on this subject, nor would it be quite correct to receive their statements. The number of patients admitted with acute disease from the better classes of mechanics was extremely small, unless the subjects of it had been reduced to poverty by previous intemperance. Most of the blacks, like others of their race, were

employed as mere day-labourers, chiefly masons' labourers, and stevedores. The poorest classes, whatever might be their occupation, were evidently more exposed to the disease than those who were richer, chiefly, perhaps, from their crowded and ill-ventilated rooms, as few of them had actually suffered from want of a sufficient supply of food.

Colour.—The proportion of deaths amongst the black men was much greater than amongst the whites, thus of the whites, one died in $4\frac{2}{3}$; amongst the blacks, one in $2\frac{1}{2}$. Amongst the women the reverse was true; thus, one white woman died of $4\frac{2}{3}$; but only one coloured woman in $6\frac{1}{2}$ nearly. These two results would, therefore, appear contradictory, unless explained by other causes.

Age.—Twenty-two patients, eleven male and eleven female, both white and coloured, were admitted under the age of twenty, (from ten to twenty years,) of these not one died. Youth, then, was almost a safeguard against the danger of the fever, and this charmed age (from ten to twenty) was as free from the danger of petechial fever, as from most other causes of death. From the age of twenty to thirty, of women there died one in $5\frac{1}{5}$, and in men nearly one in 4. It should be recollected that the largest proportion of deaths amongst the men of the age above-mentioned, occurred in blacks, who notoriously underrate their ages, especially the men who were employed in occupations and under circumstances which rendered it difficult for them to fix their dates as accurately as the women, many of whom were domestic servants. Amongst the whites the mortality of the men between the ages of twenty and thirty was only a twelfth, but amongst the women it was as high as a fourth. Therefore, after making allowance for the causes of errors alluded to we shall have but a small difference in favour of women under the age of thirty. The deaths amongst the women above the age of thirty were one in five nearly. The influence of treatment upon the mortality will be afterwards noticed.

Pathological Anatomy.—Dr. Pennock and myself examined a very large number of the bodies of those patients who died of the fever. Indeed, during nearly the whole epidemic scarcely a single examination was omitted, excepting in cases where it was impracticable from the removal of the body by the friends, immediately after death, or where putrefaction supervened, as it sometimes did almost immediately after dissolution. *In this large number of autopsies, amounting to about fifty, there was but in one case, and that doubtful in its diagnosis, the slightest deviation from the natural appearance of the glands of Peyer.* In the case alluded to, in which there had been some diarrhœa, the agglomerated glands of the small intestine were reddened and a little thickened; but there was no ulceration and no thickening or deposit of yellow puriform matter in the submucous tissue. The disease of the glands resembled that sometimes met with in smallpox, scarlet fever, or measles, rather than the specific lesion of dothinerteritis. In all other cases, the glands of Peyer were remarkably healthy in this disease, as was the surrounding mucous membrane, which was much more free from vas-

cular injection than it is in cases of various disease not originally affecting the small intestine.

The mesenteric glands were always found of the normal size, varying, as in health, from the size of a small grain of maize to three or four times these dimensions. With the exception of a slightly livid tint, common to them and the rest of the tissues, they offered nothing peculiar either in consistence or colour.

The spleen was of the normal aspect, in one half the cases, in the other half it was softened but not enlarged, and in one case out of five or six, enlarged and softened.

Thus, the triple lesion of the glands of Peyer, mesenteric glands, and spleen, constituting the anatomical characteristic of the dothineritis or typhoid fever, although sought for with the greatest care, evidently did not exist in the epidemic typhus. Indeed, it was a subject of remark, that in the typhous fever, the intestines were more free from lesion than in any other disease accompanied by a febrile movement. This exemption extended to the large intestine until the summer heats began, when a few scattering cases offered some symptoms of diarrhœa, during the prevalence of an epidemic dysentery; and, where they terminated fatally, softening, and other signs of inflammation of the mucous coat of the colon were observed.

The fact that the morbid changes pathognomonic of dothineritis, are not met with in the typhous fever, would of itself seem conclusive that the two diseases are no more identical than pneumonia and pleurisy. Although, in some respects, the two affections are analogous, and even similar, the radical difference of anatomical lesions is at least as well marked as the distinction between the symptoms. It is, indeed, singular that there should of late be a strong tendency to confound two fevers, which were regarded as entirely distinct by some of the older physicians. The prominent symptoms and difference of treatment being particularly well pointed out by Huxham.

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I have prepared for publication a case of dothineritis, and one of malignant remittent, or as it is called in southern states, congestive fever. These cases occurred during the past summer, when the epidemic of typhus still continued. The appearances observed on dissection did not differ from those already described in this Journal, in the report of cases which I published in the year 1834. In the dothineritis there were inflammation, and ulceration of the glands of Peyer, with a diseased condition of the mesenteric glands, and of the spleen. In the case of remittent fever, the lesions were limited to softening and enlargement of the spleen and liver, and inflammation of the mucous coat of the stomach.

As their publication would extend the length of this memoir beyond the limits of a Journal, I refer the reader to the cases reported in 1834. The fevers differed in their symptoms, as well as in their anatomical lesions. The symptoms of these different affections will be compared in the concluding part of this memoir, at present, and I shall consider the anatomical lesions only, which were as different in each

form of fever as the pustules of smallpox are unlike the eruption of measles. The anatomical characters of these varieties of fevers are peculiar to themselves, and it is as impossible to substitute the lesion of the follicles of the small intestine observed in the typhoid fever for the pathological phenomena of typhus, as it is by treatment or other means to transform the eruption of measles into the pustules of smallpox.

We shall hereafter inquire if the symptoms are equally distinct and characteristic in these fevers, which, from an abuse of names, are so often confounded with each other.—*American Journal of the Medical Sciences*, Feb. 1837.

Peculiarities in the Feet of the Javanese and Dyaks.—The Javanese seamen surpass Europeans, in agility in climbing the rigging, and indeed in this point are only inferior to monkeys. One of the principal causes of the superiority of the Javanese to the Europeans in working aloft, consists in the use which they make of their toes. In climbing a rope, instead of swarming up, the rope is grasped both by the hands and between the two first toes of the foot, and is ascended hand over hand like a ladder. On one occasion I saw a whole ship's company thus ascend the rigging to reef topsails; the the ratlines (the small ropes which are fixed across the shrouds like the steps of a ladder) had been taken down; but this circumstance created no difficulty, and they all got up nearly as quickly as if the ratlines had been there. A seaman when splicing a rope, or sewing a piece of canvass, holds the article between his toes; and a carpenter steadies, in like manner, a piece of wood that he is shaping and therefore retains both hands at liberty to handle his tools.—*The Eastern Seas*, by Mr. Earl, 1837.

“The Dyaks (aborigines of Borneo) are of the middle size, and well formed. * * * Their feet are short and broad, and their toes turn a little inwards, so that in walking they do not require a very wide path. The native paths are consequently found very inconvenient by an European traveller; on my visit to the gold mines I obtained practical experience of this fact; the paths used by the Dyaks and Chinese being generally worn down several inches below the surface of the soil, and as they are very little wider than the foot, pedestrian exercise proved both painful and fatiguing. The Chinese guides told me that I should soon become accustomed to these by-ways, from which I judged that the settlers had adopted the native mode of walking, with one foot before the other, since their arrival in the country, and I was the more inclined to this supposition, from the circumstance of the two interpreters having soon knocked up; one of them a native of China, a stout paunchy man, must have been particularly distressed, since he was in the habit of walking with his toes much pointed outwards.—*Ibid.*”

Oriental Craniology.—“The Dyaks (inhabitants of Borneo) are Pagans, and have many singular articles of belief, but that

“portion of their creed which obtains the greatest influence over their mode of life, arises from a supposition which they entertain that the owner of every human head which they can procure will serve them in the other world. The system of human sacrifice is, upon this account, carried to so great an extent, that it totally surpasses that which is practised by the Battas of Sumatra, or, I believe, by any people yet known. A man cannot marry until he has procured a human head, and he who is in possession of several may be distinguished by his proud and lofty bearing, for the greater number of heads which a man has obtained, the greater will be his rank in the next world; and this opinion naturally induces his associates to consider him entitled to superior respect upon earth.” The necessity of obtaining a human head to grace the marriage rites, forms a check to the increase of population never before thought of. This superstition accounts for the perfection to which the art of preserving human heads is carried in many of the islands of the Eastern Archipelago, New Zealand for example; and it likewise explains why travellers are so frequently decapitated in those countries. The chiefs sometimes make excursions of many hundred miles for the purposes of surprising and slaughtering the inhabitants of remote villages, in order that they themselves may be assured of having a numerous body guard in the next world. The heads thus obtained are dried and brought home by their captors, and are then stuck up in the most conspicuous places about their houses.—*Ibid.*

Snake Charmers.—A man of this profession in Egypt, named Mustapha, was frequently seen to take up the most poisonous snakes by the tail or any part of the body without receiving any injury, though they were neither deprived of their poison-bag nor of their fangs; and the reptile, when moving to seize his hand, was observed to withdraw his head as if wanting courage to bite it; but the same man was careful not to allow it to approach his face or legs. And this precaution being confined to those parts, suggested the probability of his hands being smeared with some substance whose smell was intolerable to the snake, and thus served as a protection from his bite.—*Lane's Manners and Customs of the Modern Egyptians.*

Egyptian Dancing Madness, and Fire-eating.—Professor Hecker has written a valuable and elaborate history of the dancing madness that seized multitudes of religious fanatics in the middle ages, and of which the name is still preserved in our nosology, under the title of *St. Vitus's dance*. The effects of various positions and motions of the limbs and body on the mind have not yet been studied by physiologists with all the attention the subject deserves and requires. That attitudes and postures exert a very important influence on the mind may be proved by the effects of the manipulations used by the practisers of animal magnetism, and by the testimony of actors, who acknowledge that it is difficult to assume the posture indicating any passion, without feeling more or less of that particular emotion. We cannot throw ourselves into the attitude of the striking combatant

without feeling somewhat of the ardour which would give strength to his blow ; neither can we imitate the shrinking posture of the terrified, or the headlong flight of the pursued, without partaking more or less of their fears. To a certain extent this circumstance, combined with the contagious nature of fear, may explain the difficulty of rallying troops once they have turned their backs to the enemy ; and even the bravest and best disciplined soldiers, in retreating leisurely before an advancing foe, find it a task to proceed in good order. The attitudes of the female dancers at Gades, described by Martial and Juvenal, and those of the Egyptian public singing girls called *Ghawazee*, exert an influence over the passions not only of the spectators but of themselves. Some dances consist of motions calculated to excite an amorous, some a martial spirit. The latter are the chief favourites of barbarous, the former of more polished nations ; and without fear of giving offence, we may be permitted to rank the waltz among the physiologically erotic species of dancing, although we do not quite agree with Byron in unconditionally reprobating its introduction amongst the English. Again, among the ancients, the value of forms in encouraging feelings of devotion or respect, seems to have been fully understood, and certain postures were accordingly scrupulously enforced in the ceremonies of religious worship, or in the respects paid to kings and princes. Hence the different values attached in different parts of the world to prostrations and genuflexions, when a subject approaches his sovereign ; matters which the unthinking regard as mere idle ceremonies, but which the physiologist must consider as founded on the fact, that these positions do actually increase the awe felt on such occasions. The priests and priestesses most celebrated among the ancients, never thought themselves inspired, never ventured to utter oracles, even at Delphi, until they had worked themselves into a frenzy by a quick succession of forced attitudes and grimaces. In Grand Cairo, at the public festival of the Mohhaaram, and others kept periodically, the whole population of Cairo, says Mr. Lane, is on the move, when the crowding, jostling, and pushing in the narrow streets and in the mosques is quite intolerable. " At these times the convolving " and dancing dervises are performing their tricks over every part of " the town, blasphemously bawling out the name of God, and asking " charity in the terms of the Koran." Mr. Lane says, " that each " seemed to be performing the antics of a madman ; now moving his " body up and down ; the next moment turning round ; then using odd " gesticulations with his arms ; next jumping, and sometimes screaming ; in short, if a stranger observing them, were not told that they " were performing a religious exercise, supposed to be the involuntary effect of enthusiastic excitement, he would certainly think that " these dancing dervises were merely striving to excel each other in " playing the buffoon." We cannot agree with Mr. Lane in this opinion, and have no doubt that the motions of the frantic dervises, properly analyzed, would be found essentially different from those of a buffoon.

Thus, says the writer of an article in the *Quarterly Review*, they dance and whirl till they become as crazy as our own

Irvingites, with their gibberish, howlings in an unknown tongue; but the feat performed by one of these enthusiasts, is so surprising, that we must transcribe it: "In the middle of this ring was placed a small chafing dish of tinned copper, full of red hot charcoal; from this the dervise just spoken of, seized a piece of live charcoal, which he put in his mouth; then did the same with another and another, until his mouth was full; when he deliberately chewed these live coals, opening his mouth wide every moment to shew its contents, which after a few minutes he swallowed; and all this he did without evincing the slightest pain, appearing during the operation and after it to be even more lively than ever. The other dervise, before alluded to as half naked, displayed a remarkably fine and vigorous form, and seemed to be in the prime of his age. After having danced not much longer than the former, his actions became so violent that one of his brethren held him; but he released himself from his grasp, and rushing towards the chafing dish, took out one of the largest live coals, and put it into his mouth; he kept his mouth open for about two minutes, and during this period, each time he inhaled, the large coal appeared to be of almost a white heat; and when he exhaled, numerous sparks were blown out of his mouth; after this he chewed and swallowed the coal, and then resumed his dancing."—*Ibid.*

A Charm put to the Test.—A circumstance occurred to this poor woman, which is strongly characteristic of the blind superstition of the natives in this part of Africa. She imagined that she possessed a charm which rendered her invulnerable to all edge tools and cutting instruments; so positive and so convinced was she of the efficacy of her charm, that she voluntarily assented to hold her leg whilst some person should strike it with an axe. The king on hearing this determined to try the power of her charm, and desired a man to take an axe and see whether this wonderful *maghony* would protect her from its effects, considering that if it did so, such a charm would be of great advantage in war. Her leg was laid on a block, and a powerful blow given below the knee, when to the poor woman's great horror, and the terror of all present, her leg flew to the other side of the room. She survived it, and now crawled about on her knees.—*Narrative of an Expedition into the Interior of Africa, by Laird and Oldfield.*

African Cosmetics.—The Felatah Ladies are very particular in adorning and ornamenting their persons. Their toilet occupies them several hours, and preparations are commenced the night before by laying the leaves of henna, moistened, to their toe, finger-nails, and hands. On the following morning the leaves are removed, the parts being stained of a beautiful purple colour. They have an extraordinary practice of staining the teeth with the acid of the goora nut and indigo, by which a blue colour is produced; a yellow dye is produced by mixing the goora nut with a small

shrub; the four front teeth of the upper and under jaw are dyed, one of a blue, the next of its natural colour, the next purple, and the next yellow! The eyelids then are pencilled with the sulphuret of antimony, and the hair, plaited in perpendicular knots four or five inches long, is bedaubed all over with moistened indigo, so as to resemble a blue helmet in appearance.—*Ibid.*

Guinea Worm.—*Dracunculus* is a very frequent disease in Western Africa, and every trifling degree of inflammation of the skin, such as a boil, is considered by the Negroes as Guinea worm. An instance occurred in the person of the Krooman named Straw Hat. He had had a boil on the upper part of the foot, and supposing it to be a guinea worm, he applied some escharotic herbs to it, which still more inflamed the part, and produced excessive sloughing of the tendons. On the ligaments of the toe becoming exposed, the man passed a piece of strong black thread round two of them, (supposing the other to be a worm coiled up,) near the instep, and then secured a piece of lead on the outside, for the purpose, as he said, of dragging away the worm. This morning I insisted on seeing it, and found that several inches of tendon had actually come away. In order to convince him of his error, I procured a fowl's foot, and by pulling the back tendon brought the other into action; I then dissected the front part of the foot, and exposed all the ligaments.

After this explanation Straw Hat seemed quite satisfied. I might have talked a twelvemonth to him to no purpose, and if I had not had recourse to ocular demonstration, I should never have succeeded in convincing him of his error.—*Ibid.*

Surgeon Oldfield, one of the authors of the work from which we have made the preceding extracts, was of great service to the expedition by means of the reputation his skill had acquired among the natives; one of his most interesting remarks on their prevailing diseases is, that cataract is extremely common, and deprives multitudes of sight. In Egypt and Northern Africa loss of vision is generally caused by inflammation, commencing in the conjunctiva; but we believe that cataract is not there prevalent in more than the usual degree. If this fact be confirmed by succeeding observers, an interesting question will arise as to the causes which render cataract so much more frequent among the inhabitants of the banks of the Niger, than among those of the banks of the Nile. Will the difference of race account for the fact?

Obesity in Africa.—It was a subject of remark among us, and occasioned some amusement, to see the different effects of heat on different constitutions. Sometimes with the thermometer at 84, I felt cold in a blanket dress, and at other times at 75 I was oppressed with heat; it appeared, however, to depend much on the moist or dry state of the atmosphere. I found that a very simple rule had hitherto kept me in excellent health; if I felt sleepy after a meal, I considered it a gentle hint from my stomach that I was

over-working it, and reduced my fare accordingly. In fact, I thought the less one consumed the better, *as all our party appeared to have a most unaccountable propensity to become fat.* I did not eat one-half that I had been accustomed to do in England, and yet could not keep myself from increasing. Dr. Briggs was precisely in the same way; and as for Lander, he was as broad as he was long!—*Narrative of an Expedition into the Interior of Africa.*

Singular Origin of Fever.—At day-light on the 22nd, we commenced breaking out the main hold, preparatory to lightening the vessel of all superfluous stores, before proceeding up the river. We had been frequently annoyed during our passage out by a disagreeable vapour that came from the hold, and we now found that it had been occasioned by the cocoa being stowed in bags in the provision-room under the cabin. The bags had rotted; this, I am of opinion was the principal cause of the unhealthiness of the after part of the vessel; the two fatal cases of fever having occurred in the poop, and no severe cases in the fore-castle where the men are much more crowded together.—*Ibid.*

Superior Healthfulness of Iron Ships.—*The Iron Steam Vessel the Alburkah, was much healthier than the larger Steamer the Quorra.* It was predicted in England, that the heat of a tropical sun would bake alive her unhappy crew as if they were in an oven; but the result was very different. Being in herself a good conductor of caloric, she was always at the temperature of the water in which floated.—*Ibid.*

African Ague.—My mornings were generally passed in reading, but in the afternoons I was generally disabled by an attack of ague. The paroxysm generally came on at noon, and each successive one was invariably half an hour later until it arrived at six o'clock, when it would leave me for two or three days, and then return to begin again at noon. This order was repeated so often in my case that I could tell to a minute when it was coming.—*Ibid.*

African Poisoning Scene.—The following *poisoning scene* presents some remarkable points of resemblance to similar scenes enacted by the enlightened Athenians, and the effects of the drug and of exercise and water in accelerating its action will strongly remind the reader of the death of Socrates—an interesting fact when we recollect that history points to Africa, as the source of Grecian civilization. The composure of the two sufferers seems likewise to have rivalled that of the celebrated sage of antiquity, although based on less philosophical grounds; this too may form a subject worthy of deep reflection.

“I was witness to-day of a poisoning scene, which it would appear “is a favourite punishment at Fundah. The culprits were two “women, who were placed under a tree in the court yard, and an “old man beat up the leaves of some herbs in a sort of mortar, the

“women sitting quietly looking on. The liquid which was of a greenish colour, was poured into two calabashes, and the women drank it off without any apparent reluctance. They then commenced walking up and down the court, drinking large quantities of water from a calabash placed in the centre of it. In about half an hour they both began to stagger and totter in their walk; and in a few minutes more the tragedy was ended by their falling flat on the ground and expiring apparently in dreadful agonies.”—*Ibid.*

We must conclude these extracts from a Narrative of a Journey into Africa with a pathological *dictum* of King Obie, which ought to be prefixed to the next edition of the works of Abernethy and Broussais; his sable majesty's paraphorism is “*When belly sick, bad palaver, very bad palaver!*”

Misunderstandings produced by Patients.—Such is the narrative. Now the lesson we would wish to draw from it consists in impressing upon our readers the caution—the extreme caution—with which they ought alike to make and to receive communications regarding the previous treatment of patients. The confidence of the medical attendant is constantly violated, and no practitioner is safe who, in his intercourse with those who consult him, does not keep constantly in mind that what he says will be repeated by the patient, whenever it suits his purpose to do so, and probably with mistake or exaggeration. We appeal to every man who has been but a few years in practice, whether this is not the case—whether, as a general rule, the public shew any good faith towards their professional attendants—any consideration whatever for the mutual feelings or points of etiquette which may be supposed to exist among medical men? Whether, in short, there is any safeguard against the most confidential observations being repeated, excepting only where it is the personal interest of the patient himself to keep them secret?

We have no reference to the mere facts of the preceding case in these remarks as to who was right and who was wrong, but we allude exclusively to the question as a general one. The correspondence referred to has very strongly forced upon our attention the difficulty under which the want of candour and the gossiping propensities of their patients often place medical men; and we, therefore, seize the opportunity of directing their attention to the circumstance. Nothing is more common than to be consulted by a party who gives his own colouring to his previous state and previous symptoms—tells us a certain portion of his history and no more—suppresses one part and colours another—probably draws from us an opinion which does not altogether tally with one he has formerly got from some other attendant, and having done this he represents, in his own way, the nature and extent of the difference, as well as the language in which it has been expressed—and behold “a very pretty quarrel” is the consequence.

That there are jealousies and rivalships in our profession, as in others, all the world knows; the *odium medicum*, we fear, is no ima-

ginary passion; but of this we are quite sure, that a great portion of the resulting evil may be traced to the mistakes or misrepresentations of the mutual patient. Nay, in the particular instance before us, and on Mr. Bushe's own shewing, we perceive strong evidence of this. A lady, he tells us, had weaned her infant "*without using any means to prevent the febrile action which sometimes take place from that cause.*" Under these circumstances—that is, at a time when she required certain remedies which had not been administered, she applied to another medical man who at once prescribed them. Now we put aside altogether what was actually said on this occasion, and limit ourselves to this, that the simple fact of administering different remedies implies a difference of opinion; and we know from experience, that patients are not backward in coming to this conclusion—nay, they frequently go a step further, and, as we have already said, in order to justify themselves for leaving an old attendant, attribute to the new one expressions much stronger than he has actually used, and very possibly add some to which he has used nothing in the least analogous.

Again, we shall suppose that a patient obtains the opinion of a medical man as to his case, and that it really is essentially different from what has been given by another: can anything, we ask, be more unjustifiable than to proceed straightway to the house of the first adviser, and repeat certain expressions, which, if used at all, must have been used in confidence? If this is to be done, how can such patient expect ever again to obtain the free and candid opinion of any medical man.

Let it not be supposed that we would countenance or extenuate a piece of unprofessional conduct, come from what quarter it may; but in the present instance there is no evidence of any such having been committed, and our only motive for alluding to it is to avail ourselves of the opportunity of putting prominently forward, in connexion with a particular case, what we have long been convinced of—namely, that half the differences among medical men, with regard to points of etiquette occurring in practice, depend upon the disingenuousness of patients who, when they wish to change their attendant, do not stick at a trifle to excuse themselves to him, and will rather attribute the circumstance to something else than the simple motive of their love of change, or loss of confidence. He who listens to the tale should at least keep in mind that a statement, the making of which involves, *ipso facto*, a breach of confidence, ought always to be received with distrust.

The longer we live the more we are convinced that medical men generally give their patients credit for more discretion and good faith than practically they will find them to display; at all events, so much of what they say is misunderstood, or misapplied, that they cannot be too careful in avoiding any expression which can by possibility be converted into censure of the medical man who has preceded them; for when the patient has changed his "Doctor," he jumps with his own conceit to be told that he whom he has discharged was undeserving of his confidence.—*Medical Gazette*, Aug. 12, 1837.

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PART I.
ORIGINAL COMMUNICATIONS.

ART. IX.—*Observations on Fever.* By ROBERT LAW, M.D.,
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MEDICINE is often charged with being an uncertain, an unsteady, capricious art, and when flippancy would indulge itself at its expense, it puts into the physician's mouth, *on a change tout cela*. So far from seeking to deny the charge, we admit it in its fullest extent, but lay it mainly at Nature's door, which, by varying disease, has imposed upon medicine the necessity of accommodating itself to its diversified character. We must even assert, that medicine and medical science have been more retarded in their progress by attempts to impose upon them a fixedness and steadiness of character, of which they do not admit, than by any other circumstance. When we reflect upon the principal sources of disease, we find them to be such as are in themselves changing, and variable, and therefore it is natural to expect that their effects would exhibit the same changing character. Among these sources we would assign a prominent

place to the influences of the atmosphere in the midst of which we live, for although chemical analysis can discover no difference between the most salubrious and the most unhealthy atmospheres, on the score of their endiometrical properties, still the striking differences which the diseases of one year exhibit, when compared with those of the preceding and subsequent ones, furnish incontestible evidence, that the atmospheric constitution cannot always be the same. If ever there was a time when atmospheric influence forced itself upon our notice, as an active agent in producing and in modifying disease, it has been within the last few years. Within this period, not only have we been visited by diseases to which we had hitherto been strangers, but every known and familiar disease has exhibited itself under an aspect of malignity quite new to it. Thus, in addition to cholera and influenza, (which, although not a completely novel disease, yet spread in such a way, and assumed such a degree of severity, as were quite unusual with it,) erysipelas, malignant scarlatina, diffuse inflammation of the cellular membrane, prevailed to an unprecedented extent. The cases of modified small-pox now, also, crowded upon us; and it was now that fever began to prevail, and we may say, that for more than two years Dublin has been the seat of an epidemic typhoid fever. For although during all this time the disease has not always existed to the same extent, nor has it maintained a uniformity of severity, still the cases have ever been more numerous than ordinarily; and whatever modifications they may have undergone, they have always observed the characteristic features of the prevailing epidemic.

It is well known that there never was a period in the history of medicine, when the exact pathological nature of fever was more a subject of controversy than at present; nor did more difference of opinion ever prevail upon the subject.

The first grand question is, whether fever be essentially connected with, and dependent upon palpable and organic lesion; or in other words, whether all fevers be properly symptomatic

or not. The next question is, as to the seat or locality of fever: various have been the situations and organs that have been selected as its proper seat, and as various have been the denominations that have been affixed to the disease. Thus has it been designated gastro-enteritis, entero-mesenteritis, follicular-enteritis, dothineritis, &c. &c.

Deeply interested in these questions, from a feeling of their great practical importance, we determined to avail ourselves of the occasion to try and ascertain the exact truth. We confess we were in no small degree surprized, at the commencement of the epidemic, to see in how many cases, and to what an extent actual disorganization, sometimes amounting to sloughing of the structure of the termination of the ilion, and of the glands situated there, was found to exist. We thought within ourselves, could such lesions have existed in former fevers, and from inattention have been overlooked? It was hard to conceive. Again, we remembered, that although no where is fever more prevalent, and, perhaps, no where more fatal than in our city, and consequently no where is there more opportunity of investigating its pathological nature; so few were the pathological specimens of this ulcerated condition of the small intestines and the mucous glands in the museum of the College of Surgeons, that our valued friend Mr. Houston, the Curator, set especial value upon one with which we had an opportunity of furnishing him a few years since, nor had this specimen any other pretension to his favour than its rarity, it being, we believe, the only one he then had. Had the lesion been nearly as common as fever, whose victims principally supplied the dissecting rooms, we cannot suppose it possible that it could have escaped the close observation of the intelligent pathologist, of whose talent and zeal the rich pathological collection in the museum of the College of Surgeons is a proud monument.

In the course of the last year, we had an opportunity of conversing with M. Andral on the subject. Although he did not seem altogether decided, that the intestinal lesion was the

essential anatomical condition, or the *sine quâ non* of fever, still it was quite obvious that the frequency of it had given a decided bias to his judgment. We reminded him of our having followed his visit with M. Lerminier, at La Charité, about twelve years before, when we had observed many cases of typhoid petechial fever, and had witnessed not a few *post mortem* examinations when this lesion was not noticed. He conceived it possible that it might have existed, but have been overlooked from attention not being particularly directed to it. We found it very difficult to acquiesce in the opinion, that so accurate an observer of pathological appearances, whose patient industry we had so often witnessed and admired, could overlook any thing even like the disorganization that later investigations brought under our notice in similar cases, and especially too, at a time when he must have been engaged in collecting the material for his valuable work on pathological anatomy. M. Louis answered us more decidedly, stating it as his unvarying experience, that, whenever in a case of fever he found the abdomen tympanitic, the surface marked with lenticular petechiæ, and a sense of gurgling or gargouillment when the ilio-cœcal region of the abdomen was pressed, then he was sure to discover, on examination after death, an affection of the glands situated towards the termination of the ilion. We are found so far to consent to the opinion of M. Louis, that we have met with extremely few cases of ulceration of the intestine, in which there did not exist tympanitis, petechiæ, and ilio-cœcal gargouillment; still we have met with all these symptoms in the most aggravated and the most rapidly fatal cases of typhous fever, when examination after death could detect no trace of intestinal lesion. M. Bouillaud, whose clinique is more crowded than that of perhaps any other physician in Paris, teaches that typhous fever is an entero-mesenteritis, and treats it with a degree of activity and energy, which we own we should not have felt ourselves warranted in pursuing; nor have we reason to regret the results of a mode of practice diametrically opposite to what an active inflammatory

disease would demand. In many and the most aggravated cases of typhus, the single and simple indication which presented itself to us was to feed the expiring lamp of life; and in not a few of such cases, wine has been the only remedy we have employed from the time they have come under our care, till we have pronounced them convalescent.

The epidemic which still exists, and which we before stated has existed for now more than two years, has brought under our notice almost every modification of fever, not only such as depended upon individual peculiarity or idiosyncrasy, but under all the varied aspects impressed upon it by change of season. Thus in winter and spring, have we had it accompanied by bronchitis, and that not of the nature of that which alone indicates itself by sibilant rale, and which we should perhaps overlook, did we not apply the stethoscope to the chest, and which is so often to be met with in fever, that Laennec regarded it as constant, but so extensive and so aggravated, as literally to produce suffocation by the copiousness of the effusion into the bronchial tubes, as well as into the parenchyma of the lungs. Some of these cases closely resembled Armstrong's Congestive Fever, and on examination of the bodies after death, we generally found the lungs both anteriorly and posteriorly in such a state of engorgement or congestion as we ordinarily only find in the posterior part, and which we designate cadaveric congestion. In such cases the congested organ generally resembled the softened spleen, and admitted of being easily torn. Pneumonia complicated some cases, and exhibited to us some of the most striking specimens of suppurative pneumonia, or in its third stage. The local affection generally came on very insidiously, indicated often neither by cough nor by dyspnoea; nor did it exhibit the same predilection for the inferior part of the lung, that ordinary pneumonia does: we as often detected it at first under the clavicle as elsewhere. These cases have tended to confirm us in an opinion which we have ever entertained of pneumonia terminating in suppuration, that it deserved rather

to be considered as allied to diffuse inflammation of the cellular membrane, than to true genuine pneumonia. The constitutional symptoms and local appearances or pathological characters seem to me both to warrant this opinion. The accompanying fever is generally of the typhoid type, and the anatomic characters are the same as we find in this species of inflammation, viz. effusion of lymph into the cellular tissue of the organ, and purulent infiltration into the substance of this effused lymph. At the same time that we met with this complication of fever and suppurative pneumonia, we also met with cases of phlegmonoid erysipelas, and nearly about the same time a remarkable case of laryngitis presented itself to us. Almost all the symptoms of fever had subsided, we had in fact pronounced our patient convalescent, when, at our morning visit, we found her in the greatest distress from difficult respiration. She had been seized with sore throat during the night. The inspirations were long, hoarse, and croupy; there was tenderness on pressing above the thyroid cartilage. Inspection of the fauces discovered nothing unusual, but the epiglottis could be felt hard and thickened; pulse 160, not strong; we had her largely leech-ed in the throat; blistered the nape of the neck, and directed calomel with a view to affect the mouth and as soon as possible. We strenuously persevered in these means, and her distress seemed considerably lessened, still the pulse never was lower than 150 in the minute, nor could the system be brought under the influence of mercury, although, in addition to the calomel, we had the blistered surface dressed with mercurial ointment. At the end of five days she expired. The integuments of the throat were very much swollen, and the parts hardened and matted together; this, however, seemed to be the effect of the leeches and blisters that had been applied in this situation. The epiglottis was very much thickened and indurated. The sides of the rima glottidis were so approximated, that it resembled a cleft made with a knife. All the parts were very much thickened, and required a degree of force to cut into them. When an incision was made into the thickened parts, there was

a thin purulent matter in the midst of the substance which caused the increased bulk of the parts. This substance was lymph, effused into the submucous reticular cellular tissue. It occupied the tissue which is more ordinarily the seat of œdema. The disease was strictly confined to the larynx; there was no trace of its extension to the trachea. The left lung exhibited a considerable extent of engouement, closely approximating to red hepatization. The subject of this case was a woman of naturally vigorous constitution, aged twenty-six, and as far as we could ascertain, was not exposed to any thing likely to produce the untoward complication. The point to which we attach especial interest in this case, is the lesion which we believe has not been noticed. The unequivocally inflammatory nature of it distinguishes it from the serous effusion into the same submucous tissue, constituting œdema of the glottis, and to relieve which, antiphlogistic means are so often unsuccessfully resorted to. It obviously differs, also, from the inflammation consisting in the effusion of lymph on the free surface of the mucous membrane, or croup: nor does it owe its rareness so much to the lymph effused into the sub-mucous tissue, as to the purulent infiltration into this lymph, which is the pathological character which suggested to us its identity with diffuse inflammation. The incidental mention of this case has brought to our mind other cases of laryngitis, which we met with in the course of the epidemic, not, however, exactly like this one, yet not altogether different, "*facies non omnibus una, haud diversa tamen.*" In one of them, there was a large collection of purulent matter forming a tumour in front of the throat, opposite the pomum Adami. The matter diffused itself into the cellular membrane connecting the muscles at each side of the neck, especially about the os hyoides, and the root of the tongue. Large masses of lymph were thickly laid on all the mucous membrane of the larynx, almost completely obstructing the rima glottidis. This lymph could be easily detached from the surface on which it is deposited. The subject of this case was a man of sixty years of

age. Although we have many cases of laryngeal inflammation connected with fever, and coming on at a time and under circumstances which did not lead us to expect it, yet the limits of an article of this nature will not allow us to do more than merely to glance at a few of them. We would here allude to a case of erysipelas of the head and face, in a female aged sixty, of not very temperate habits. When the swelling of the face was subsiding, and the eyes had become open, the throat became affected. Both respiration and deglutition were laboured and painful. Extreme prostration and collapse of the system took place, and death quickly followed. Mr. Porter, in his judicious observations on Laryngitis, has noticed the similitude between erysipelas affecting the mucous membrane of the larynx, and diffuse inflammation. We would further observe, that we have remarked the tendency of laryngitis oedematosa to occur in, and to be the fatal termination of cases of constitutional irritation. This has been observed in a case of glanders in the human subject, the symptoms of which were, in other respects, the same as the effects of a dissection wound, or of the introduction of any animal poison into the economy. It is worth while to remark, that during the prevalence of epidemic typhous fever, the fever accompanying the exanthemata, (which, perhaps, prevailed to a greater extent than we had ever before seen them within the same period,) in many cases assumed the same typhoid type. This was especially the case with scarlatina and erysipelas; the former particularly exhibited itself under this character, and literally carried off entire families; the latter, too, often created great apprehensions for the safety of those affected by it, and most so when it affected the head. It often arose spontaneously, but more commonly was caused either by blisters or proceeded from bed sores, against which, in many cases, the most vigilant cautions could not provide. Not only have we seen these sores to proceed from a part being pressed by lying upon it for a very short time, but also have observed them to be produced by the legs being crossed, the weight of the upper one

acting upon the part of the one upon which it lay. We hold in mind one case, in which not a part that pressed the bed, even the sole of the foot which was applied to the foot-board of the bed, escaped ulceration. Professor Graves remarks, that when a bed sore forms, both doctor and nurse should be dismissed, intimating that their prevention is, in all cases, within the reach of the attendant's care. We have met with some cases of their occurrence, in which we feel perfectly satisfied that no vigilance of ours or of the nurses could have prevented them. These were cases, the subjects of which were of a gross, leuco-phlegmatic habit. When, in such cases marked by constitutional inelasticity and deficiency of vitality, the brain and nervous system become oppressed, this untoward circumstance occurs, and in an inconceivably short time. We have observed the same to take place in cases of apoplexy, and obviously from the same cause, viz. the nervous influence being withdrawn, by reason of the pressure on the brain. Not only is the slightest pressure, and that even for a very short time, competent to produce slough and mortification of parts in some instances, but we shall find the same to occur in parts which have not been subjected to pressure. We had a remarkable instance of this in a female who was convalescent from mild fever. At our morning visit, we unexpectedly found her in bed, complaining that she felt herself very unwell, that she had had a bad smell, which she could not get rid of. Her countenance wore an expression of deep distress and anxiety; the features were drawn, the point of the nose was quite purple and cold; she seemed shrunk within half her natural size, and had very much the wretched appearance which we sometimes observe with a person in the cold stage of intermittent fever. There was a complete collapse of all the powers of life; no stimulants could rouse her; she sunk in a very short time. We have had occasion to observe the tips of the ears mortify independently of pressure, and have seen the consequent ulceration the *point du depart* of erysipelas of the

head and face. This we found a very embarrassing case, as conflicting indications seemed to present themselves. Delirium generally accompanied the erysipelas, yet the condition of our patient being generally that of extreme prostration, controlled us in the use of any but a nicely modified plan of antiphlogistic treatment. This, in general, consisted in the exhibition of a small portion of tartar emetic in a stimulant menstruum. We also gave wine at the same time, and in some cases rather freely, which, so far from aggravating the delirium, had a decidedly contrary effect.

In summer and autumn, the epidemic assumed more the character of gastro-enteritis. It completely swallowed up the English cholera and dysentery which usually visited us at this period of the year. It now exhibited those features of character which foreign pathologists are disposed to regard as essential ; and, indeed, so constant were those features at this period, that scarcely a case was met with in which they were not to be seen. Diarrhœa was a most frequent symptom, and it was now that we had most frequent occasion to observe that most untoward symptom, intestinal hæmorrhage.

Towards the close of the year, the epidemic seemed to merge, for a time, into influenza ; they seemed, at least, to modify each other, the latter assuming the typhoid type of the former.

At the commencement of the present year, the epidemic again re-appeared, and so extensively, that the government felt themselves called upon to provide additional hospital accommodation to meet the emergency. It also appeared under a character of malignity, which it had not exhibited before ; it terminated fatally within a shorter period than it had done before, the patient, in some cases, exhibiting something like an apoplectic seizure. Now it was that we especially had occasion to observe those complications, which bespoke the lowest state of vital energy, viz., different modifications of diffuse inflamma-

tion, such as purulent deposits in different parts of the body, suppuration of the joints, mortification of parts, purulent pneumonia, &c. Yet under these circumstances we completely lost sight of ulceration of the intestine, not an individual instance of which have we seen for several months, till within the last week, when we witnessed two, each of which exhibited perfect specimens of the particular lesion of the small intestine and of the mesenteric glands. These cases are somewhat uncommon: in neither were there petechiæ; in one only was there tympanitis. One had diarrhœa, which yielded to a few grains of Dover's powder. Both, certainly, were in a state of extreme prostration. The intellect in each was perfect, although in one it was dull and slow. One died on the sixteenth, the other on the twenty-third day of his illness. The extent of organic mischief was greatest in that which proved fatal on the sixteenth day. The morbid appearances were of precisely the same character in both. The stomach and duodenum and jejunum were healthy. We had also got through a considerable portion of the ileum before any unusual morbid appearance met our eye. We first saw small round bodies, not larger than grains of small shot, studding the mucous membrane, and of the same colour as the membrane. Then were to be seen small circular ulcerations, with elevated margins, somewhat resembling variolous pustules. As we approached nearer to the termination of the intestine, the ulcerations were larger, situated nearer to each other; affecting various forms, some circular, others more of the form of an ellipse, with their longer diameter transversely, and exhibiting at their bottom the denuded muscular fibres. At the termination of the intestines, were large sloughy patches, of considerable dimensions, and with jagged shreds of cellular membrane, coloured with bilious excrementitious matters. These sloughy ulcerations existed on the ilio-cœcal valve, but did not extend into the cœcum, nor was there any thing unusual, with the exception of a little increased vascularity of the mucous membrane of the large intestine. The mesenteric glands in each case

were enormously enlarged, and when cut into exhibited a striking resemblance in their substance to that of the kidney, when unusually vascular. The enlargement and vascularity of the glands plainly bore a direct proportion to the degree and intensity of the disease of the portion of the intestine nearest them. We observed the ulceration of the intestine always to occupy the convex portion of the intestine, or the remotest portion from the mesentery. Although we have constantly forty or fifty cases of fever under our care, these were the first instances we have had of this lesion for nearly a year; a lesion which, before that period, was the most common morbid appearance in fever.

From these cursory and general remarks on the epidemic fever, we shall notice a few of the most constant and striking features that characterized it. We feel no hesitation in setting down the presence of *petechiæ* as the phenomenon more rarely absent than any other. The most common period of their appearance was from the fifth to the eleventh day. Hitherto we had been in the habit of observing them in general only associated with a *Cortege* of symptoms so indicative of a bad and dangerous type of fever, that to pronounce a person in a spotted fever, was considered nearly tantamount to signing his death warrant. But in this epidemic we met with this phenomenon occurring in a fever so mild as scarcely to demand the interference of art.

The only cases in which *petechiæ* were not present, were those of children. We do not remember to have seen a case of fever in a child under twelve years of age in which they were to be seen, although in not a few instances have we observed purpura hemorrhagica. This modification apparently dependent upon age, occurred to us as more remarkable when we reflected how much more disposed childhood is to cutaneous affections, that it is, in fact, the proper period of exanthemata. We conceive this circumstance is explained by the opposite condition of the capillaries of the skin in the exanthemata, and in cases of *petechiæ*,

they being in the former in a state of active erethism, while in the latter they would seem to be in a relaxed state. We believe them to be in the same condition of contrast, that they are in active and passive hemorrhage. The blood also in this particular kind of fever, seems to us to undergo a change consisting in a diminution of its density, which favours its passage into the relaxed vessels, and thus contributes to the formation of the petechiæ. The older physicians regarded this dissolution or breaking up of the blood as putrefaction, and to correct it, directed the medicines which they designated antiseptics; among which they assigned the foremost place to the acids. Although we deny that this altered consistency of the blood is putrefaction or like it, yet we conceive it to be similar to the effect which some poisons are known to produce upon this fluid, changing its consistency, and depriving it of its power to coagulate. We know that this fluid state of the blood is the only unusual appearance generally met with in the examination of bodies when death has been caused by lightning, and in other cases in which the nervous system has been the portion of the animal economy principally affected. The petechiæ, so constant in the epidemic, presented various appearances. In some instances they so closely resembled measles, that had we the eruption alone to determine our judgment, we should not have found it an easy matter to decide between measles and petechial fever. We had frequent opportunities of noticing the presence of sudamina, which although in most cases they did not exceed millet seeds in size, yet in some were large phlyctenæ. They did not seem to be accompanied with any symptoms, and were generally met with when the skin was relaxed in perspiration. This epidemic exhibited this striking difference, when compared with other preceding ones, the complete absence of anything like crisis. We feel ourselves safe in asserting, that we have scarcely seen a case in which there has been a decided crisis, at least such a one as we constantly observed in the epidemic of 1826-27, when we were almost sure to have a

marked change on the fifth or seventh day. Now the disease gradually progresses till it has reached its acme, and then as gradually declines, so that we are ever kept in a state of tremulous anxiety till convalescence is perfectly established. The fever has in general been protracted. From the circumstance of the patient seldom seeking admission into hospital till their disease had begun to assume somewhat of a dangerous character, we found it difficult in most instances to fix the exact duration of it, but as far as we could ascertain, twenty-one days seemed to be its average. We had a single instance in which the patient was actually five-and-thirty days under our care before she exhibited any sign of amendment. She was reported to have been a fortnight ill before admission into hospital. This was the most protracted case of fever we had ever witnessed, in which there was no incidental or accidental inflammation to account for its unusual duration. In this case, no doubt, there was intestinal hæmorrhage, but in no other case where we had observed this phenomenon, did we see the disease equally protracted.

Derangement of the sensorium was a sufficiently constant feature of the epidemic, under its various phases, to deserve to be noticed. At the early period of the epidemic, delirium, and that of the most violent kind, came on under circumstances that we were but little prepared to expect it. Individuals came under our observation with symptoms of mild fever, and with nothing in their appearance to give rise to an apprehension that any thing untoward would occur, when in the midst of the profoundest calm, a delirium came on, in some cases so violent that mechanical restraint was required. This occurred in the case of some individuals whose habits of temperance were above suspicion. In some instances the patients had been three or four days in hospital before this took place. That this should be the case in persons of irregular habits was not surprising, and did occur very frequently. In fact, many of these cases so closely resembled delirium tremens, that we should have regarded them

simply as such, did not the presence of petechiæ identify them with the prevailing epidemic. This sudden outburst of violent delirium sometimes came on in the midst of symptoms denoting the worst and lowest stage of typhus. Among these cases we had two remarkable instances in two sisters situated in different wards; their fevers pursued exactly the same course in reference to the delirium; it came on equally insidiously in each, and in each was equally violent. These were the cases in which we derived so much benefit from the combination of tartar emetic and opium, generally exhibited in a cordial stimulating menstruum. Among the cases of this nature, accompanied with marked cerebral excitement, were some puerperal cases. These we found in general to yield readily to the combined tartar emetic and opium. Two of such cases strikingly illustrated the effect of this combination. The individuals came into hospital under such excitement as to require to be placed under restraint. They were reported to have been in this excited state for several days, during which they had no sleep. In each case tranquil sleep supervened shortly after the second dose of the following mixture, was taken.

℞ Mist. Camphoræ ℥vii.

Tart. Emetic. gr. ii.

Aceti Opii gutts. xxx.

Spirit Etheri Nitrici ℥iii.

Syrupi Croci ℥v.

Misce, sumat unciam tertiis horis.

In one of those cases the delirium came on seven weeks after delivery. In the interval of delivery and the accession of fever, the individual was affected with mammary abscess, which we suspect, by acting as a counter-irritant, kept the disposition to mania in abeyance; for no sooner had the abscess healed than the mania supervened.

Although perhaps it would be more regular to reserve our observations upon a point of practice to a later period of our remarks when we come to consider the treatment, still we cannot

leave the subject without noticing the great and decided advantage we found to follow the use of the shower bath, either cold or tepid, in some of those cases of high cerebral excitement, in which we had in vain tried to tranquillize or procure sleep for our patient, either by opiates, or the combined tartar emetic and opium. In many cases the patients themselves have solicited a repetition of the bath, from a consciousness of the benefit they derived from it; and often have we seen sleep to come on almost immediately on their getting to bed. In not a few instances the violence of the patient has been such, that the strait waistcoat could not be taken off while he was getting the bath, but immediately afterwards the necessity for it had ceased, and in consequence it was removed. Besides the delirium which came on thus early in the course of the fever, and in some instances, it was amongst the earliest symptoms, and gave the disease very much the character of meningitis, we found it to come on at later periods; often from about the eleventh to the fifteenth day we noticed this phenomenon; and from observing it to come so frequently, when diarrhœa, tympanitic abdomen, ilio-cæcal, gargouillement and red tongue pointed to affection of the intestinal mucous membrane, we looked upon it as the sympathetic effect of this affection: and we were soon taught that the abdominal affection required very cautious and delicate management; for we remarked, that when the diarrhœa was suddenly checked, an immediate aggravation of the cerebral symptoms was the almost certain consequence. It seemed as if the disease were then exclusively concentrated upon the brain. Hence, we recognized the necessity of applying our treatment to the two sets of symptoms, and with this view we applied at the same time leeches to the head and abdomen, behind the ears, and along the side of the neck; and to the epigastrium, (where both the stomach and the transverse arch of the colon shared in the advantage,) to the ilio-cæcal region, and to the verge of the anus. Besides these means cold lotions were applied to the forehead, cataplasms to the ab-

domen. In some cases, either blisters or turpentine stupes were applied to the abdomen. The medicine upon which we placed most reliance in such cases, was the combination of hydrargyrum c. cretâ, and Dover's powder, which we never found to check the diarrhoea prematurely. Before we leave this subject, we feel the necessity of subjoining a caution as to the employment of tartar emetic, with a view to control the delirium, arising under the peculiar circumstances which we are at present considering. The state of the bowels plainly contra-indicates it: still we have seen it used, and have seen the intestinal affection much aggravated by it. We have thus, not unfrequently, had occasion to observe, how the unseasonable employment of a valuable remedy will entail discredit upon it, because it has not realized ungrounded and unreasonable expectations, expectations based upon the erroneous idea, that the same phenomena or symptom is ever to be dealt with in the same way, no matter what be its cause; an error which will appear still more glaring, when we pass on to the consideration of the delirium which comes on at a still later period of the disease.

One of the strangest circumstances connected with disease is the fact, that two conditions, not only different but diametrically opposed to each other, will give rise to the same phenomenon; thus do we see delirium at once the result of determination of blood to the brain, as well as of a deficient supply of blood to this organ, as in cases of profuse hæmorrhage. Were this symptom alone, irrespective of its cause, to guide our treatment here, into what a mistake would we be betrayed? But a protracted disease, producing extreme debility and exhaustion, will have precisely the same effect as the anemic state of the brain, it will equally give rise to delirium, and equally require a modified treatment. The best illustrations of this have been furnished us by cases of erysipelas coming on in the progress of fever. If other circumstances warranted us in the practice, the delirium, so far from deterring us from giving wine, was in fact a reason for our doing so. Although we could adduce number-

less cases to confirm the fact, that the delirium under these circumstances disappears as the other symptoms improve under the exhibition of wine, still we feel that it is one of the points of practice demanding a degree of judgment equal at least to any other of medicine. Indeed in many such cases the exhibition of wine is an experiment, the effect of which the physician should himself narrowly watch, and not leave a discretion with an attendant, who can scarcely be expected to be competent to appreciate the contingencies which should cause the stimulus to be increased or diminished, or to be entirely withdrawn. This consideration leads us to express our persuasion, that the safety of a patient in bad fever peremptorily demands the repeated visits of the physician through the day ; for changes occur so suddenly, and require such immediate attention, that often a moment lost is irretrievable.

We often find delirium in the form of some particular delusion or hallucination, surviving every other symptom of fever, and according to its character, whether agreeable or otherwise, modifying the convalescence. Thus have we seen the private soldier pluming himself on his imaginary elevation to the rank of Captain ; and the pennyless pauper exulting in the fond delusion of having become a wealthy legatee, so that when the unlucky blister, applied to the nape of the neck, brought them back to "all the horrors of sobriety," each, if he did not say it in the precise language of the poet, at least thought

" Pol me occidistis, amici,
Non servastis."

Again, we have seen convalescence actually retarded by a delusion of a different complexion. Thus have we seen a poor woman so possessed with the feeling that her husband was in confinement for dishonesty, that no means we could employ to persuade her to the contrary had the least effect. All the symptoms of fever were revived, till we had her husband brought, when every thing went on smoothly. We have seen

imaginary pecuniary losses produce the same effect. A blister to the nape of the neck will in general (not always, time alone effects it in some cases) set all to rights.

Of the contagious nature of this fever we have had abundant evidence in the way in which almost all the members of families have been affected, not only those residing under the same roof, but also those who lived separately and at distances, but amongst whom intercourse was kept up. We had a remarkable instance in seven members of the same family, but residing in three separate localities, being affected, although not at the same time. First the old grandmother became affected and died; then her daughter-in-law, living in a different part of the city; then her two married daughters, one of whom died; then the husband of this last, and at the same time two children; last of all the grandfather, aged 70, was brought into hospital almost moribund; he, however, recovered.

We alluded, in an early stage of our observations, to the occurrence of diffuse inflammation in some cases of this fever. We have had several instances, and had reason always to regard it as a most fatal complication. It exhibited itself at an advanced period of the disease, in the form most commonly of tumefaction of the joints, sometimes with a slight erythematous blush. The knees, ancles, and wrists were the most common seat of this affection. In one case of scarlatina this inflammation affected the cellular membrane of the neck, and involved the sterno-clavicular articulations. In another instance, it extended from the shoulder along the neck to the anterior mediastinum, and the anterior surface of the pericardium. In other cases we found collections of purulent matter in different parts of the body, not raised into a tumour, but causing sometimes a scarcely appreciable fulness of the parts where they were situated. The constitutional symptoms, which were generally present, were extreme prostration of the powers of the system, delirium, small weak pulse, diarrhœa, tympanitic abdomen, and in some cases an indescribable anxiety, while in others the patient

told you he felt himself much better. On examination of the parts affected with inflammation, the tumefied joints, in general, contained purulent matter of a thin, greenish, unhealthy character, and in some cases, the cartilages were either in whole or in part destroyed, leaving the ends of the bone denuded and rough. In some cases the tumefaction was owing to the infiltration of a gelatinous fluid into the cellular membrane about the joints. When the matter was found in other parts, it was not confined in a cyst, nor was there any hardening of the parts from effused lymph in its vicinity; it was in the strict sense diffuse inflammation.

We would observe that this inflammation occurred in many instances after a decided amendment in the symptoms of the fever had taken place, and when we had every reason to expect a fortunate issue. This leads us to make a general observation, that in an epidemic the mildest case is not without danger, nor is the extremest case without hope. Our prognosis cannot be too guarded—for we know not the moment when appearances calculated to excite the most sanguine hopes, may be disappointed; and, again, we have seen cases, where the principle of life seemed to be only not extinct, recover under the use of stimulants. We might remark that the period of the epidemic when this untoward complication of diffuse inflammation presented itself was when *post mortem* examination discovered no affection, at least ulceration, of the intestinal mucous membrane, and certainly it was the period when the deaths were most numerous.

An article of this nature precludes our entering much into the detail of treatment. Indeed, most of our observations upon this point have been anticipated in the progress of our remarks already made; we must, however, give somewhat of a sketch of our usual practice.

The simple principles upon which we early learned to treat fever, and which seem to us to comprehend almost, if not altogether, all the indications that can present themselves, are, 1st,

to diminish the actions or functions which are in excess ; 2nd, to increase those which are defective : 3rd, to restore the due order of succession of those which have had it disturbed. We shall first remark on the last of these. As the cause of fever, whatever it may be, or whatever be the direct mode of its operation, changes healthy and natural into unhealthy and unnatural action ; to invert this operation by originating a new train of actions analogous to those of health, is the principle of cure. It is the early period of disease alone, before the morbid habit is formed, or the organization of parts is injured, or taken the impress of unnatural action, that the morbid chain can be snapped across consistently with safety ; and now decided practices, of whatever description, succeed, and complete and perfect recovery of health is often the effect of directly opposite means. It rarely happens that a case of fever presents itself to us till the period when we might have hoped to have accomplished this object has already gone by. We have, therefore, to apply the two first principles of treatment to the unhealthy or unnatural actions in the progress of their development.

Let us now consider the application of these two principles to the prominent features of fever, for instance, to the derangement of the circulation, and of calorification, which are the two functions most commonly and most prominently affected in fever. The action or motion of the heart is either unnaturally increased, or in cases unnaturally diminished. The obvious indications are, either to diminish or to increase the unnatural action, not in all cases by the same means, but by means accommodated to the peculiar circumstances of the individual case. Thus, in one case we lower the circulation by bleeding, in another case by purgatives, in another case by sedatives, &c. &c. We may apply the same principles either to the elevation or to the lowering of the temperature, and so, in fact, deal with almost all the unhealthy actions or functions, the results of the febrile morbid principle. And although we affect not to say, that all the treatment of fever will yield a ready submission to these

simple rules, we can only say, we have felt the advantage of their application in treating the disease. True, much connected with disease lies beyond our ken, nature has placed a *velum interpositum* between the precise form of morbid action, and our means of ascertaining it; still there is much which can be ascertained, and much too which happily can be righted.

We shall notice but very few points of practice. There is no point upon which medical men have differed more than upon bleeding in fever. We shall only observe, that we were led to give it a very fair trial, in an epidemic of a much more sthenic type than this present one, and from the result, see no reason to adopt it as a general practice. On the contrary, we are quite sure, that we have observed injurious effects from its being resorted to only because the disease was fever.

In the treatment of epidemic fever, and indeed of all epidemics, the fact of the accompanying fever being almost always of a typhoid type, has not been sufficiently attended to, and in consequence much injury has resulted from a treatment being adopted, too vigorous for an epidemic, but which could have suited a sporadic case of the same designation. In the recent influenza, especially when it assumed the character of broncho-gastric fever, we have met with some cases which we know to have been seriously injured by active depletion. We conceive that bleeding should not be employed in fever, unless there be some serious complication, and in general in epidemics, when we can safely, and without temporizing, substitute local for general bleeding, we ought to do so. It is needless for us to advert to the different parts to which we apply leeches in fever, these of course must depend upon the particular seat of congestion, and to where the blood is determined. We would only remark how much relief some patients have derived from their application to the nape of the neck, where they had been distressed by headach, especially affecting the occiput, and from their application along the spine, where pain and uneasiness were referred to this region, as they are so constantly to the loins.

We found young females who had become affected with fever, when the menstrual discharge had been either defective or absent, suffer from most distressing pain, and sense of fulness of the head: with these, leeches applied to the feet, and stupes afterwards, yielded great relief. We seldom had occasion to open the temporal artery. Shaving the head, application of cold lotions to it, and leeches, generally superseded the necessity. We would here testify our complete assent to an observation made by M. Andral, on cold lotions applied to the head, viz. that their continued application has sometimes an injurious effect, producing torpor of the brain. This effect we have seen exhibiting itself in a general chill, cold feet, weak pulse, &c. We therefore in most cases direct the occasional sponging the head.

Did the limits of the present article permit, we should gladly detail some cases to illustrate their exclusive and entire treatment by stimulants. In many of these cases the patients were brought under our care with the entire surface completely covered with petechiæ, in some instances with a cadaveric coldness, and a pulse scarcely perceptible. Here the single and simple indications were to support the ebbing powers of life, and to restore the temperature. Some of these cases resembled so much cases of cholera, as far as the failure of the circulation and calorification went, that had they occurred at a time when this disease prevailed, they might easily have been mistaken. Most of these cases recovered by a free use of wine, and in some cases of brandy; with these we associated our hospital formula of cardiac mixture composed of camphor julep, carbonate of ammonia, and Hoffman's anodyne. It was quite remarkable the quantity of stimulants some cases actually required before we could feel a pulse at the wrist. The difficulties of the case only now begin; it is now that discernment is required to regulate stimulation. Hitherto there was no fear of doing too much. Now the apprehension arises, lest action be exerted beyond the powers of the system to sustain it, and be followed by a degree of languor proportionate to the degree of the pre-

ceding excitement, still the impression made must be supported. To regulate stimulation, to come up to and not go beyond its due point, not only exhibits the nicest exercise of medical skill, but in many cases demands the closest vigilance and watching. For in many cases it is utterly impossible to gauge the precise quantity of the stimulus which may be requisite, its effects alone being the regulating measure of it. This is, perhaps, the point of all others which, in the treatment of fever, imposes on the physician the necessity of seeing his patient at short intervals. When we have made good our ground by stimulants, our object now is to withdraw them, but not suddenly. With this view, in addition to diminishing the quantity of wine, we dilute our cardiac mixture ; then substitute for it camphor mixture, with aromatic spirit of ammonia ; then for this we substitute effervescing draughts with aromatic spirit of ammonia. A modification of this treatment is what we found applicable to the generality of cases. We tried the chloride of lime, and soda, but cannot say that we found them answer the expectations which the accounts of their great benefit in fever would have led us to expect ; nor did they, when exhibited in the early period of the appearance of petechiæ, seem to check their further development.

Although here, perhaps, would be the occasion for us to make some observations on the exhibition of opium as we employed it, we feel the subject would require us to say more than would consist with the limits of this article. We would also observe that it is, perhaps, one of the points of delicate management of fever, upon which it is extremely difficult, if not utterly impossible, to lay down any approximation to general rules ; experience, and it alone, will suggest its timely exhibition.

We have already adverted to the combination of tartar emetic and opium, exhibited in the delirium of fever. For a series of years we had employed this combination in the treatment of delirium tremens ; and in some cases of fever with delirium of somewhat a similar nature we had also used it with like success ; but about two years since it was, that multiplied cases of fever, exhibiting a striking similarity to delirium tremens, af-

fording us an opportunity of more extensively employing it, and of being satisfied of its value. We cannot afford space for more than a mere allusion to what we have more fully detailed in a clinical lecture delivered by us in Sir Patrick Dun's Hospital, and reported in the *London Medical Gazette*, No. 40, June 2nd, 1836. We there remarked, that observing irritation and congestion to constitute as it were the two pathological elements of the cerebral affection in delirium tremens, we were led to employ a combination of remedial agents, whose influence would be directed against each element of the mixed affection, and apportioned the quantity of each ingredient of the combination to the degree in which the indications of irritation or congestion predominated. The combination we employed consisted of tartar emetic and opium, the former directed against the congestion, and the latter against the irritation. When the irritation and congestion seemed to co-exist in an equal degree, the ingredients of the combination were adjusted accordingly. When the irritation existed alone, or if there were any congestion, it was so inconsiderable as scarcely to shew itself, a mere fraction of tartar emetic was admitted into the combination, the opium being proportionate to the irritation. When, again, the congestive symptoms were so prominent as to mask those of irritation, then the tartar emetic was employed, nearly, or altogether to the exclusion of the opium. These conditions seemed to embrace almost all the modifications that were presented to us whether in delirium tremens or fever, and we found that a careful adjustment of the remedies met the indications. In two cases which have just been under our care, we exhibited the tartar emetic and opium in a stimulating menstruum, with most marked advantage. Tranquil sleep, after most protracted absence of it, followed in each case after a very short time.

Before we conclude our observations upon our practice in fever, we would remark, that although in no respect is the treatment of this disease more improved than in the exhibition of purgative medicines, still, too much of the old system of

daily purgation, and that by means of strong drastic cathartics, continues, and brings its injurious effects under our observation. There are two kinds of fevers, in which, especially, we have noticed pernicious consequences resulting from this practice ; one, in which the gastro-intestinal mucous membrane has a tendency to inflammation, and in which, although a diarrhoea most commonly exists, still not always ; and when it does not, a purgative will be sure to produce it ; the other is a low nervous fever, in which the operation of even a mild cathartic is attended with a considerable degree of physical exhaustion. The judicious treatment of this last consists more in withholding than in giving medicine.

The post mortem appearances that we have had occasion to observe have been very various. In some cases, and in which head symptoms predominated during life, we have not only met with the congested state of the large superficial veins of the brain, the opaque thickened appearance of the arachnoid, an unusually dotted condition of a transverse section of the organ, but have seen as extensive an effusion of fluid into the ventricles and upon the surface of the brain, as if they had been cases of pure hydrocephalus.

In cases where the chest seemed to be principally engaged during life, we found in general the parenchyma of the organ, to a greater or less extent, in a state of engouement or congestion, varying from a dark blackish appearance resembling a softened spleen, to the reddish vermilion hue of red hepatization. In general, the bronchial membrane was highly injected, and you could easily follow the bronchial ramifications, exhibiting this vascular condition of their lining membrane, into the congested parenchyma in which they were lost. When pneumonia had existed in its third or suppurative stage, more or less of the organ was solidified, was of an ash gray colour, and easily admitted of being broken down. There was a sparing exudation of purulent matter, when a portion of it was pressed. In some cases this change of structure had invaded nearly the whole of the organ, in others it was confined to a single lobe ; but, our

general observation was, that it more frequently engaged the upper part of the lung ; and this we had occasion to observe in many cases that completely recovered. We are at a loss to conjecture, why the apex of the lung should in this case, as in phthisis, be the favourite seat of the lesion, and not the base or posterior part, as in ordinary pneumonia ; another reason, with many, to convince us of the essentially different nature of the affection from common pneumonia.

Some cases, in which there was obstinate vomiting during life, exhibited the mucous membrane of the stomach more or less injected ; and in one case in which this symptom could not be controlled, this membrane exhibited an universal suffusion or crimson blush, and was so soft and pulpy that it could be easily detached and scraped off. The aberrations of this membrane from its normal state, were more in its consistence than colour and vascularity.

The duodenum seldom presented any unusual appearance. The remainder of the small intestine was generally more or less congested, the capillaries situated in the loose submucous tissue easily admitting of that congestion which was every where to be seen, and which appears to us as much entitled to give the designation of gastro-enteritis to fever, as the injected state of the capillaries of the skin would be to give it the designation of dermatitis.

When the mucous membrane of the small intestine has been deeply engaged, we can in general detect the situation of the lesion before we open the intestine, by a dark appearance which it presents externally, and by a thickening which is palpable when the part is felt between the fingers. The lesion is generally confined to the last portion of the ilion. We first detect a patch of small glands clustered together, and affecting various shapes and forms, sometimes longitudinal, sometimes circular. These clusters of glands or follicles presented very much the appearance as if the portion of the mucous membrane, where they were situated, was raised by some substance placed underneath it, and

was then perforated by a pin. This seemed to be the first stage of the lesion, for this was the appearance which we met with in cases in which death had supervened very quickly, before time had been allowed for the complete disorganization or lesion. When we approached nearer to the termination of the intestine we observed elevations resembling various pustules with small ulcerations in their centres; then we met with true ulcers, either circular or elliptical in their shape, with the denuded muscular fibres plainly to be seen at their bottom. The nearer we approached the termination of the intestine, the nearer were these ulcerations to each other. They most commonly occupied the convex portion of the intestine, in reference to its attachment to the mesentery. Next to the cœcum, and on the margin of the ilio-cœcal valve there was generally a large, irregular, sloughy ulcer, commonly besmeared with bilious, excrementitious matter. It was sometimes quite astonishing to witness the extent to which the mucous membrane had been destroyed, and how little notification there had been of it during life, and also to see how near the ulcer was in some cases to perforating the intestine, without doing so. In general the vessels of the mesentery were very much injected, and the glands situated near the ulcerated intestine large, and when cut into, presented a red fleshy appearance. The congestion generally extended to the cœcum, but not much beyond it.

We would here advert to a singular appearance presented in the case of a woman aged 40, who seemed never to rally after the first moment of her being seized with fever. There was a complete collapse of all the vital energies; the appearance to which we allude, consisted in an infiltration of air into the sub-mucous tissue in different parts of the small intestine, from the duodenum to the ilion. The body exhibited no other remarkable appearance. Was this putrefaction, or decomposition, anticipating dissolution? In almost all our examinations we were struck with the fluid condition of the blood. These morbid appearances were what we most frequently met with,

yet we could adduce cases in which all the most prominent features of the most aggravated typhus were present, and still examination could discover no lesion upon which to charge the severity of the symptoms. We would conclude our remarks by observing, that if a close and careful examination of this subject has not taught us what fever is, it has at least taught us what it is not; that it is not gastro-enteritis, nor entero-mesenteritis, nor dothineritis, but a disease sometimes modified by local lesion, which is an accidental complication, but not an essential condition. Or if local lesion be essential, it must be confessed it is of such a nature as in many and the worst cases to elude observation and detection. It has also allowed us to see that it is rather impatient of the restraints that systematic medicine would impose upon it, and will not submit to them. It further tells us that the physician who would really understand disease, must be satisfied ever to be a learner; that he can never fold his arms and complacently rest upon his stored up stock of information; he must be ever adding to it; he must ever continue to read Nature's page, which is ever changing and ever varied. Medicine and medical science have at least this attraction, that they have always something new wherewith to repay the labours of their votaries.

ART. X.—*Observations on the Use and Abuse of Mercury.*

By CHARLES LENDRICK, M.D., T.C.D., King's Professor of the Practice of Medicine, Clinical Lecturer in the School of Physic, Physician to Mercer's Hospital, &c.

IN my former observations on the use of the *nitro-muriatic acid bath*,* I dwelt especially on its efficacy in those venereal cases, where circumstances prohibit the administration of mercury. I shall now proceed to consider more in detail the operation of the latter powerful remedy, premising that there is much

* See Dublin Medical Journal for May, 1837.

which I have learned from experience, that others have derived from a similar source, and perhaps communicated ; so that I lay no claim to originality. It is my object to direct *attention* to several important practical points, which, from the variety of occupation, may be insufficiently considered by many members of the profession.

A controversy exists at the present time, (see the last number of the Medical Journal,) as to the claims of mercury to the name of a *specific* in venereal cases. I do not think that the cause of medical science has been much advanced by dwelling on *names* of any kind ; more especially as we often find that disputants about *words* do not very materially differ as to their ideas of the things signified. That mercury is adapted to all forms and stages of the venereal disease, I believe no one at the present day will contend for ; but although it has a very marked influence in other diseases, its effects in *favourable* cases of syphilis are so much *more* so, as to render the term “ specific ” *almost* applicable.

Describing the *mercurial disease*, Mr. Mathias says, (page 32,) “ It is a disease that mostly takes place during the course of another complaint, or at least when that complaint is under cure, and therefore very liable to be confounded with it. In its symptoms it strongly resembles the previous disease.”

Mr. Hunter and the late Mr. Todd of Dublin, have graphically described two distinct and opposite operations of mercury on the constitution ; the one characterized by its visible effects on the secretory and vascular systems, the other on the nervous system, the former *generally* beneficial, the latter always injurious. From these *extreme* influences of mercury, we might, even in the absence of more precise information, anticipate a considerable variety of *intermediate* operations on the system, producing changes beneficial or the reverse, according to the form and stage of existing *disease*. As to the effects of mercury during a state of *perfect* health, and freedom from both general and local disease ; it is a fact upon which our information is necessarily defective, as it is no easy matter to find per-

sons fool-hardy enough to use mercury for the mere purpose of experiment, or of enabling pathologists to record the mode of its operation.

The symptoms of the morbid operation of mercury on the system, are described faithfully by the authors I have mentioned, and by others. In many cases, however, where mercury does not disagree sufficiently to display those symptoms in the *constitution*, or where they have not yet had time to develop themselves, it manifests its operation on the *weak part*, that labouring under, or predisposed to disease; and this generally with a force proportional to its salutary influence on that disease where mercury agrees with the system. If the warning from this aggravation be not taken in time, and mercury be persevered in, the *constitutional* symptoms of its morbid influence become manifest, while the disease for which it was administered assumes a new form, and becomes nearly, and sometimes altogether uncontrollable by remedial measures.

For instance, the salutary effect of mercury in some diseases of the *liver*, especially in hot climates, is so universally conceded, as to nearly entitle it to the name of a specific as far as they are concerned. Yet there are many affections of this organ, and even stages of that in which it is usually beneficial, where, instead of removing or preventing disorganization, it proves its *cause*, and the patient sinks into a state of marasmus, generally followed by death.

Dr. O'Beirne* has directed the attention of the profession to the powerful efficacy of mercury in acute affections of the *joints*; yet how often do we see painful swellings of the articulations, with copious effusion into their cavities, *ensuing* on the administration of mercury for other diseases, and where the predisposition to any affection of the joint was previously so slight as scarcely to have attracted attention.

There are, perhaps, few diseases which receive more de-

* Dublin Medical Journal, May, 1834.

cided advantage from the judicious administration of mercury, than *dysentery*, and yet, if the morbid influence of the remedy should display itself during the treatment of this disease, it will prove its aggravation instead of its cure. The reader will find this circumstance referred to by the late Dr. Cheyne,* and it is remarkable, that in the cases where mercury proved prejudicial, there was evidence, from the state of the gums, and from constitutional symptoms, that an *anomalous* action of the remedy had taken place in the system at large.

Many other instances might be adduced of this somewhat *homœopathic* action of mercury, which is not, however, peculiar to it. Among other remedies, opium presents an exemplification. Opium is one of the most powerful means of counteracting diarrhœa; yet in that depraved state of the constitution appertaining to *opium eaters*, diarrhœa is one of the most usual symptoms. From the great variety of diseases, however, in which mercury is applicable, its influence this way is especially marked; and as it is found that the more specific the beneficial operation of mercury on a disease is, the more decided are its morbid effects; we cannot be surprised that the phenomena are more clearly developed in *syphilis* than in most other maladies.

It would, I think, considerably tend to clear our ideas on the operation of mercury, to bear in mind that its effects on a large scale in quicksilver mines and elsewhere, clearly entitle it to the denomination of a *poison*, although a more slow one than arsenic or other minerals. This consideration does not militate in the case of the one more than the other, against the operation being beneficial where it is kept within bounds, and applied under favourable circumstances. It shows, however, that we ought not to be so much surprised as we frequently are, at mercury *disagreeing*; its disagreement with the human constitution being, in fact, the rule, and its favourable opera-

* Dublin Hospital Reports.

tion the *exception*, and not *vice versâ*, as we are too apt to suppose.

In administering mercury in venereal, and indeed in most other cases, it is to be also borne in mind, that the gradations from its beneficial to its morbid operation are extremely uncertain. Sometimes mercury is found *gradually* to disagree, and having proved salutary for a time, it then fails, and finally proves injurious. In other cases, the transition is rapid and decided from the one stage to the other ; the disease for which it was administered having been in a progressive state of recovery, and becoming suddenly exasperated without apparently any stationary period. These facts ought to make practitioners especially cautious to select the proper stage and the proper mode of administration. A very slight error as to either, or as to the continuance of mercury for even a very short time beyond the proper period, may lead to irreparable mischief.

When the morbid operation of mercury takes place during the treatment of venereal cases, there seem to be two modes in which it displays itself.

1st. The existing symptoms, whether primary or secondary, become aggravated—and often considerably so, before there is any proof of its disagreement with the constitution. Ulceration spreads, and becomes malignant ; unhealthy inflammation, followed by gangrene, attacks the neighbouring parts, and new ulcers appear in their vicinity. If mercury be continued, and the local destructive effects are not sufficiently marked to deter the practitioner, its morbid influence on the *constitution* becomes apparent, by some of the symptoms detailed by Hunter and Todd. In some instances, however, we find, and that even in the treatment of the primary disease, *some* derangement of the general health, *preceding* its morbid action on the part, and from which we ought to take warning in time. I have known cases where the warning was *not* taken, and mercury pushed only *a little* too far, with most deplorable results. I attended a gentleman for primary symptoms some years since, and who

had previously lost a portion of the penis from sloughing during a former venereal attack, (mercury naturally disagreeing with his constitution,) and notwithstanding that the ulceration was small, and that the case was treated from the first with every precaution as to regimen, &c., the cure occupied upwards of four months, so frequently did the morbid alternate with and supersede the favourable operation of mercury; the constitutional symptoms of this morbid action sometimes taking precedence of the local, and on other occasions, an unfavourable change in the latter leading the way. No secondary symptoms ensued. Perhaps it will be said that the case ought to have been treated *non-mercurially*; to this, however, I am certain the patient would not have consented, as he said the administration of mercury, from which he before suffered, had become necessary in consequence of a previous non-mercurial attempt.

2ndly. Mercury, when acting injuriously, attacks parts *predisposed* to the venereal disease; of this the most obvious instance is, the occurrence of secondary chancres, at the close of the treatment of the primary, or while the patient is using mercury for other secondary symptoms. It also has a peculiar tendency to *accelerate* the disease in those predisposed parts, and often in an aggravated form. Thus we find symptoms called by some venereal, by others pseudo-venereal, and by some mercurial, breaking out in the throat during the treatment of a primary chancre, and long before they could be expected to occur spontaneously as secondary symptoms. Disease in the periosteum and bones, called by Hunter "the last order of parts," will often take place, and often in an inveterate form, during the mercurial treatment of what are considered as early secondary symptoms; and I have seen a chronic swelling and induration of the testicle, which is usually a late occurrence in syphilis, preceding all the other secondary symptoms. This led the sagacious Abernethy to consider all such venereal symptoms as occurred "out of their order"—as pseudo-syphilitic, or at least not to be treated with mercury. It is, however, to be recol-

lected, that the homœopathic action of mercury is only displayed in *predisposed* parts, and I quite agree with Dr. Colles, in never having witnessed nodes, or any other apparently venereal symptom produced by mercury in an *untainted* constitution; although in that contaminated by syphilis, these and other syphilitic symptoms will take place, earlier and more severely in consequence of the administration of mercury, and of its *unfavourable* operation on the system. Sometimes the favourable and unfavourable operations of this remedy are so intermingled, or the latter so slightly developed, that while mercury seems to produce new symptoms, it appears to be curing others. In this case, the safe rule is, rather not to do good, than to commit positive harm. The suspension of mercury may seem to delay the cure, but it may prevent the necessity of discontinuing the remedy altogether, and thus be the means of saving instead of losing time, by only lessening the beneficial, while it removes the morbid action.

The treatment of those venereal cases in which mercury has been either not used at all, or so slightly as to produce on the entire neither a favourable nor unfavourable effect, (and for practical purposes the cases may be considered as the same,) is plain sailing, compared with that of the disease where mercury has been abundantly used in vain. In the latter, we are surprised to find the patient with well marked symptoms, (primary or secondary,) while he has taken treble the ordinary quantity of the medicine. This surprise is the consequence of adopting too strongly the opinion as to the *quantity* of mercury necessary. Taking, for instance, three ounces of ointment, or about forty frictions as a pretty fair allowance, we find that half a pound has been used in vain. But if it be recollected, that during three-fourths of the time, perhaps, mercury was acting inefficiently or injuriously, we have a right to reckon only one-fourth of the half pound or two ounces, as being beneficially used; and *against* this, we have to set off at least *some* morbid action, aggravating old symptoms, and accelerating new ones, and

which, even under the most judicious management, must have occurred during the administration of the remaining three-fourths of the mercury. So that on the entire, matters would have been better by the use of two ounces than of the half pound. By using mercury "off and on," laying it aside with reason, and resuming it without it, it is surprising the quantity that can be crammed into *some* constitutions, doing no good, and sometimes not *much* harm.

In judging, however, as to the morbid effects of mercury, it is to be remembered that *all* its evil consequences are not necessarily displayed *during* its administration. These sometimes seem to commence weeks, or even a longer time, after it has been laid aside, and pursue an obstinate and intractable course. Under such circumstances, the patient frequently charges his medical attendant with having discontinued mercury too soon, and lays the blame on a too sparing administration of the antidote. If the latter act on such suggestions, the destruction of the former is no unusual consequence. The practitioner may rest assured, that such venereal symptoms as occur *soon* after a course of mercury, are not to be benefited *then* by its administration ; and the probability of such a dilemma as consequent on its over administration, ought to make him very careful to lay aside mercury in any *doubtful* case in time.

Where venereal symptoms are thus *brought out* by mercury, they generally pursue one of two courses, if they do not prove fatal.

1st. If mercury has been laid aside in time, and the practitioner has not confounded its aggravatory influence with the ordinary course of the disease, and especially, if appropriate anti-venereal treatment (independent of mercury) is resorted to ; these symptoms appear to subside ; and then if (as is often the case) they do not re-appear, it remains a question, whether the bane has not been also the antidote ; that is, whether mercury has not accelerated such symptoms to cure them, or at least to leave them curable by other means.

2ndly. If mercury has not been left off at the precise time, or rather, if its injurious operation has not been rather anticipated than waited for, these symptoms pursue a tedious and vexatious course. They become mitigated under non-mercurial treatment, and alternately present inducements for resorting to, and laying aside that remedy. At length the patient (if he live long enough) gives up mercury in despair, is relieved for a time, and finally submits to "a course," and is *cured*.

In judging in one of those difficult cases, where mercury has been copiously administered in vain, as to the propriety of its exhibition, there are many points to be taken into consideration, besides the state of the general health, (*independently* of syphilis,) scrofulous predisposition, the frequency of the pulse, &c. Have the symptoms broken out or been aggravated *during* the active administration of mercury? have *time*, and anti-mercurial treatment been allowed fair play, so as to strip the *complicated* disease of its *mercurial* character? If the former question be answered in the affirmative, and the latter in the negative, we may be assured the time has not *yet* come for using mercury with advantage.

In such cases, (as I mentioned in the former paper,) every effort ought to be made to relieve the patient by air, diet, sarsaparilla, acids, hydriodate of potash, acid baths, &c., so as to defer as long as possible having recourse to mercury. The longer it is delayed under such circumstances, the more likely it is to *hit*. The sooner it is resorted to, the more likely it is to fail. In administering mercury, the precautions detailed by Dr. Colles, so necessary in cases of primary syphilis, are quite indispensable here. Even in ordinary cases, it is but one step from safety to danger; but the interval becomes farther diminished, where mercury has already failed. Here the best, the *very* best mode of administration is alone justifiable. In some cases, an *alterative* course of corrosive sublimate, or of some other mercurial preparation, in small doses, *succeeds*. At any rate it is the best preparation for the fuller administration of mer-

cury, as if it be found to disagree, we may rest assured that we cannot *press* mercury at the time.* If, on the other hand, the symptoms be found to amend up to a certain point, and the patient's constitution has *improved* under the alterative use of the medicine, whilst the disease has become nearly stationary, a more full administration of mercury may be ventured on. If a slight affection of the mouth take place, it will generally be prudent to await its effect on the system and on the disease for a few days, and not to continue the mercury for the purpose of keeping up its action. In the attempt to do so in a debilitated constitution, we incur the risk of going back instead of forward in the cure. The affection of the mouth ceases, or becomes morbidly modified, and the symptoms retrograde. This first action of mercury on the mouth in *debilitated* cases is seldom considerable, and yet very important. It may be generally considered as an indication for laying the mercury aside, and watching the result, after which it may be again had recourse to with greater confidence. I know that some experienced practitioners prefer "keeping up" this first action of mercury; and the attempt may be justifiable, if the constitution will stand it, which it will rarely do in the cases I am considering. On the contrary, so sudden is the gradation *from* the healthy to the morbid action of mercury, (and, I am sorry to say, *not vice versâ*.) that an amendment of the symptoms, with a slight demonstration from *time to time* of the *sensible* action of mercury, on the system, is all we can hope to accomplish. More would, doubtless, be desirable; but in the attempt we run the risk of foundering altogether. This use of mercury may be called the *intermediate* between the alterative, and what is termed a full *course*. If the patient will not bear it, he will not bear the latter. It *may* cure the disease, or, it may go a certain way, and

* It will be seen, hereafter, that a *brief* though powerful action of mercury may be borne under such circumstances. I am here referring to the *slight constitutional* as compared with the alterative administration.

demonstrate the capability of the patient to bear a more active use of mercury. Our practice with this powerful and dangerous remedy, in such cases, must be in a great degree *tentative*.

I have before* mentioned that the efficacy of sarsaparilla, &c. in syphilis, is mainly attributable to their counter-influence over the morbid action of mercury. If so, it might be presumed, that what has a remedial, must also have a certain amount of preventive influence; and that it would be advantageous, especially in cases where the morbid action of mercury is much apprehended, not merely to exhibit sarsaparilla, antimonials, &c. whilst mercury is laid aside, but also *during* its exhibition. This practice is too much neglected in general. I have myself had recourse to it with decided advantage for many years: and I find that it is strongly recommended by an almost incontrovertible authority, Sir A. Cooper—(Lecture 65.) Dr. Wallace informs me, that he has with great success *combined* the administration of the hydriodate of potash (for the utility of which in venereal cases we are so much indebted to him) with that of mercury. Any thing which will in the slightest degree better the operation of mercury is of importance, for it may turn the *balance* between its favourable and unfavourable operation.

If we are by judicious management, by continuing mercury while *agreeing*, and by discontinuing it on any *doubt* occurring, so fortunate as to remove the existing venereal symptoms, our mercurial treatment is over for the time. New symptoms, or the recurrence of the former, may render mercury again necessary; but to continue mercury *after* the disappearance of *secondary* symptoms, is a practice inefficacious as a *preventive*; while the risk is incurred of causing a recurrence or new form of the disease by the morbid action of the remedy, if it should, as is very likely, disagree with the constitution.

We sometimes meet cases where the venereal disease appears to advance steadily during the intervals of the use of mercury,

partly perhaps spontaneously, and partly from the dregs of the mercurial action. On having recourse to mercury, it seems to receive a temporary check, but the medicine again disagrees, and the patient is worse off than ever. This happens chiefly in scrofulous* constitutions, that will not bear the mercurial action *long*, although they bear a considerable amount of it for a short time. In cases of this kind we often succeed by laying mercury aside altogether, and having recourse to non-mercurial treatment, sarsaparilla, tonics, acids, &c. and then bringing the patient *rapidly* and *briefly* under the influence of mercury. I admit that the practice is sometimes precarious, and hence every precaution should be taken to insure the beneficial action of the remedy. Blood-letting should precede, if any inflammatory action be present, together with the warm bath, cicuta, and antimonials. The patient should now be confined *to bed*, and three grains of calomel, with half a grain of opium, exhibited every four hours. If we are apprehensive of the action of mercury on the bowels, half a drachm or more of the ointment may be rubbed into each of the axillæ and thighs, (from two to three drachms in all,) night and morning. Thus the system will, in thirty-six hours probably, be brought under what is called "the salutary" operation of mercury, although it has previously only *teased* the constitution. As considerable danger might ensue, should mercury not hit, the practice is only to be recommended where mercury cannot be used otherwise, and yet cannot be done without. A mercurial salivation will sometimes remove the ve-

* Doctor Colles considers that in scrofulous constitutions mercury may be pushed so as to produce pyalism in a short time, although previously given for a long period in alterative doses in vain, or injuriously. I cannot subscribe, however, to the opinion, that the use of mercury may in general be *continued* with safety under such circumstances. Dr. Graves, in describing the mercurial treatment of *phthisis*, and some other scrofulous diseases, adverts to the distinction between *producing* and *keeping up* mercurial action in a scrofulous constitution. The success of the former in *hydrocephalus*, is a sufficient confirmation of the correctness of his views.

neral symptoms altogether, and occasionally only cause them to disappear temporarily. If they recur, they are to be met anew. On no account should any attempt be made to *keep up* the action of mercury under such circumstances, by its continued administration. So considerable a shock is given to the system by the forcible operation of the remedy, that the least *further* irritation will often sink its powers altogether.

I not long since admitted a patient into Mercer's Hospital, apparently suffocating from cynanche. I found that she had been using mercurial pills (apparently judiciously administered) for venereal symptoms. She had been attacked with sore throat, which became worse as the mercury was continued, and it had been therefore laid aside. On examination, I perceived a sloughing ulcer, which had destroyed the uvula, and a good deal of the soft palate. Topical and general blood-letting, and powerful escharotics, including *white* butter of antimony and creosote, were used in vain. Mercury having disagreed with, and almost caused the disease, I was apprehensive of having recourse to it, particularly as mercurial fumigation failed. As a last resource I treated the woman with calomel and opium, administered in the way I have mentioned. In about thirty-six hours she was in a smart salivation, and the ulcer *granulating*: she lost her haggard appearance; and even within a day or two looked ruddy and healthy. I thought, "if little was good, more was better," and continued the mercury night and morning. In a few days, however, she complained of vertigo and nervous symptoms, lost appetite, and looked ill. The mercury was at once laid aside, the morbid symptoms subsided, and she left the hospital in about a fortnight, quite well, with the ulcer in the palate healed, and the cleft, caused by the loss of substance, nearly filled up.

When a treatment of this kind causes only a temporary disappearance of the symptoms, it remains a question, whether, on their recurrence, mercury should be used in the common way, or in that which has already proved successful? There are, doubtless, constitutions which can bear better, and cases which

profit more, by these mercurial “shocks,” than by “a course.” The practice of Mr. Wallace in such cases is very judicious. He administers the hydriodate of potash on the subsidence of the mercurial action ; and sometimes thus effects a cure. On other occasions it is necessary to give another mercurial shock. Many, previously intractable cases, are cured by thus *alternating* the use of hydriodate of potash, with what I term (from its marked effect in iritis,) the *iritic* administration of mercury.

There are indeed some cases of iritis which beautifully illustrate both the morbid and the salutary action of mercury. Thus iritis breaks out under a mercurial course, and becomes aggravated, as the medicine is continued. Mercury is laid aside, and the case is treated by local blood-letting, belladonna, and the internal administration of turpentine : the symptoms subside ; the eye, however, remains liable to attacks of inflammation ; and one of these, more serious than the rest, is cured, along with the whole disease, by—mercury. This is a very important and instructive illustration, and shews, both that a disease which generally requires mercury may be *produced** by it in *predisposed* parts, and that such may be finally *cured* by it, if used at the *proper* time.

We are often embarrassed, during a mercurial course, to determine whether we are giving too little mercury, or whether mercury is disagreeing, especially if its salivary action be but slight, the doses moderate, and the case one (such as that of nodes) which often yields to the full, and not to the slight action of mercury. This is a matter of the last practical importance, as if we give more mercury on the former supposition, when it is actually disagreeing with the system, we may ruin the patient. I think the following considerations will often prove our guide :—

1st. If the disease has remained *from the commencement* of the mercurial course *unchecked*, the former supposition is, under the above circumstances, probably correct, and mercury may be *cautiously* increased.

* Mercury may cause as well as cure *dropsy*.

2ndly. If the disease *has* been checked and is *stationary*, the case is doubtful. If the mercury be now suspended, and has been previously *insufficient*, the disease will *then* gain ground, and the indication for a fresh and *more* abundant administration afforded.

3rdly. If the disease *has* been checked and is *now* getting *worse*, mercury is disagreeing, and it ought to be *discontinued*, at least temporarily; for if mercury once *acts* on the disease, it will, so long as its action is favourable, at least *suspend* it. If we have first an *amendment*, (see No. 1.) and *then* a deterioration *before* mercury is discontinued, we shall lose instead of gaining by continuing, and *a fortiori*, by increasing it.

Perhaps the day may yet come when practical principles, such as those that experience has *forced* upon us with respect to mercury in the venereal disease, may enable us to use this powerful remedy far more extensively, in many more diseases, and with much greater success than at present, and on this subject I may perhaps (God willing) again submit my views.

To Mr. Carmichael a debt of obligation is due by the profession and the afflicted, for the improvements he has introduced into the treatment of syphilis, and for rules of prognosis and diagnosis, which prove unquestionably correct in a considerable proportion of cases. Still I cannot subscribe to the opinion, that we have *data*, in the present state of our knowledge of the venereal disease, to prognosticate the result after secondary symptoms appear. On the contrary, I think the phrase "all at sea" as then applicable. So much depends on idiosyncrasy, and the chance of an unlucky hit with mercury, either by ourselves or others, (for mercury will be indispensable at *some* period,) that even the most flattering cases often turn out deplorably.

For this reason the *prevention* of the venereal disease is a subject of the last importance, and this name is, I think, assignable to the cure of the *primary* symptoms; for till both practitioners and patients can be got, as *formerly*, to look

on these symptoms as almost nothing on their own account, and almost every thing on account of their probable consequences—an efficient practice can scarcely be hoped for where the *apparent* danger is so slight. My own observation of secondary symptoms, not only establishes a conviction of their highly formidable nature, but also that they take place in a considerably greater proportion of cases, than is generally supposed.

I have long since urged on my class, in the case of every doubtful sore on the genitals, to adopt such local and constitutional treatment, during the first week from their appearance, as will enable them during *the second*, in case such has not *completely* disappeared, to have recourse to a full course of mercury. For the mode of conducting this, and the precautions to be adopted, I refer the reader to the able works of Drs. Colles and Wallace. There are many other treatises highly valuable, besides such as I have just quoted—Sir A. Cooper's lectures, and the works of Bacot, Abernethy, Mathias, Howard, &c., and though last not least John Hunter. If the latter seem tedious in the perusal, let the reader bear in mind the commentator's words, "whenever he has an *intricate* case in *his own* practice he will not accuse Mr. Hunter of *prolixity*."

I do not mean to say that the treatment of the *primary* symptoms in their *first* stage, by means of mercury fully and efficiently administered in the most favourable manner practicable, (for this is the *sine quâ non*,) might not in many cases, such as I have mentioned, be dispensed with, and yet secondary symptoms not appear: but were this the case, even in nine cases out of ten, (and this is *very* far from the truth,) the chance of one to nine in favour of their occurrence, and the uncertainty as to what they may prove if they *do* occur, is, in my opinion, decisive as to a preference for the *best* mode of treatment, while treatment is in *our power*.

It was an old opinion that mercury was "carried off" by salivation. It is needless to discuss the correctness of the ex-

pression, but it is undoubtedly true, that a certain action on the *secretions* is indispensable for its salutary operation. In doubtful cases we find that by acting on the *skin* by means of the warm bath, antimonials, or sarsaparilla, or by producing evacuations by purgatives or blood-letting, we secure the favourable operation of mercury. An opposite mode of treatment succeeds, when mercury is disposed to act *too well*, and to salivate too quickly. Here we expose the patient to a cool air, and administer opiates.

The *morbid* action of mercury is always a *greater* one than what is requisite to produce its healthy or salivary influence in the *present* state of the patient's constitution (if such be practicable *at the time*). The salivary action is the *saturation* of the constitution, and ptyalism is absent or deficient when the mercurial action is (morbidly) above, as well as when it is below *par*. The former is shewn by the effect of diminishing it, by lessening the doses, or increasing the interval between them. In this way we succeed in affecting the mouth, where large doses had previously failed, or caused the morbid action of the remedy. Dr. Colles justly remarks, that the very persons who are at one time too readily salivated, are at another difficult to affect with *ordinary* doses. The truth is, the term "resistance to mercury" is quite incorrect. In most cases too quick salivation, and the non-occurrence of salivation, arise from the same cause—*too great* susceptibility to mercury. In the former, the salivary effect takes place soon and obviously, and the mercury is laid aside; in the latter, it takes place soon, but gradually and imperceptibly—the mercury is continued, the proper grade of its action is *passed through*, the salivary tendency is lost, and the more mercury the worse. This latter practice arises from the supposition, that the "resistance" is similar to that whereby some constitutions resist the action of purgatives, and which is overcome by increasing the doses. The fact, that *this* resistance is overcome by *diminishing* the mercurial action, establishes their opposite nature. The object is still better attained by com-

bining with the diminished administration of mercury, the *secretory* treatment mentioned above. Thus the mercurial action *falls back* to the healthy standard. Authors prescribe doses, practitioners must look to effects. Idiosyncrasy, a temporary state of the constitution, or the result of *previous* mercurial treatment, may make it more difficult to adjust grains, than it formerly was to manage drachms. If we look back to those vexatious cases where mercury has disagreed, and yet where nothing else would cure, we often find a period when a *tendency* to its healthy action displayed itself. Unfortunately the practitioner, forgetting individual peculiarity in general analogy and *prescribed doses*, considers such an occurrence as encouragement to continue the mercury, which soon loses its effect. I am certain that if in many *such* cases mercury were discontinued on this tendency being observed, and the *secretory* treatment were resorted to, the favourable action of the remedy would be brought about, and might be continued if necessary.

Ptyalism subsides in the average of constitutions in about eight days, if mercury be not continued. Its continuance is resorted to principally to *keep up* the action. Salivation certainly seems to be not only the test of its healthy action, but also a kind of safety-valve, since we find that similar effects are produced on the disease, without any very marked difference in the state of the mouth, by very different doses of mercury, *after* ptyalism is once established. To this remark there are, however, exceptions. Sometimes a very slight increase of the dose, or a diminution of the secretions by cold, or even a *continuance* of mercury for the present under the most favourable circumstances of salivation, &c. will cause such local or constitutional symptoms, as denote that the excessive or morbid action is commencing. If mercury be at once stopped, no harm has been done, except that its *over*-administration has prolonged the period of cure by a few days. If it be persisted in, the consequences may be deplorable indeed, in proportion to the state of the patient's constitution, and the previous accumulation of mercury in it. I feel

convinced, that there is scarcely a case of unsubdued syphilis where mercury has been *freely* used, that it has not been pushed too far at *some* period. Ground thus lost, is difficult to *regain*.

ART. XI.—*Remarks on Inversion of the Uterus*. By THOMAS RADFORD, Surgeon Extraordinary to the Manchester Lying-in Hospital, &c., and Lecturer on Midwifery at the Royal School of Medicine and Surgery.

(*Continued from No. XXXIV. p. 25.*)

THE opinions of writers and teachers are so discrepant upon the practice which ought to be adopted in cases of inversion, where the placenta still adheres to the uterus, that it appears important to the writer to inquire into their validity. When we find modern writers of the highest authority ranking hemorrhage in these cases,* as always dangerous, and sometimes fatal, we ought to have strong grounds before we contradict those statements: but authority, however eminent, ought, and must yield to that opinion which is founded upon facts. The frequent absence of hemorrhage in this accident is a truth so certain, that the writer is astonished that any practice should have been based upon the idea of the universality of its occurrence: he alludes to the practice of not detaching the placenta, before reversion of the uterus is effected. In speaking of the absence of hemorrhage, the writer wishes it to be understood, that he alludes only to those cases which have been well managed. It would be folly to deny that bleeding may be produced in inversion, as in

* Denman, Waller's Edition; and Francis, American Ed. Burns' 8th Edit. and also James, Do. Ramsbotham, Practical Observations. Newnham's Essay. Ingleby, Obstetric Medicine. Gardien *Traité complet D'Accouchemens*, &c. tome iii. p. 309. Boivin and Duges *Traité pratique des Maladies de l'Uterus*, &c. tome i. p. 289; and also Heming's Trans. p. 119. Sir C. M. Clarke, *Observations on those Diseases of Females*, &c. p. 151.

other cases, by injudicious treatment. The tumour in this accident has been mistaken, and the placenta brought away in part, leaving a portion attached to the fundus;* the uterus has been forcibly pulled at,† lacerated,‡ cut into,§ and even torn away with the crotchet.|| In such cases it is no wonder that hemorrhage is a prominent feature.

When considerable flooding occurs during labour, it always depends upon a partial or complete separation of the placenta; but in inversion we find, generally, that the placenta is adherent. There are exceptions to this found in the published cases, but some of these are not to be depended upon.

In all cases hemorrhage is more violent, when the placenta is partially detached, than when it is completely separated; and nothing increases it so much as partially detaching, and breaking into the structure of this organ. The placenta, whilst adherent to the uterus, is in free vascular communication with it, and if a partial separation and disruption be effected, bleeding mouths are exposed. But if the placenta be completely detached from the uterus, this organ contracts as under ordinary circumstances, and the bleeding ceases. The validity of this opinion is shown on reference to those cases where the placenta was removed previously to reverting the uterus. In many cases in which hemorrhage is stated to have occurred, there is great reason to suspect that the reports have been exaggerated from alarm; and in many of these cases the accounts are second-hand. After natural labour there is a greater or less lochial discharge, some part of which may be concealed from observation, remaining in the cavity of the uterus; but in inversion it is otherwise, for the whole of the discharge must pass externally. In five of the cases connected with this paper no hemorrhage took place,

* Brown, *Annals of Med.* vol. ii. p. 278. Ingleby, *Facts and Cases in Obst. Medicine*, Case p. 223.

† Cawley, *London Med. Journal*, vol. vi. p. 278. Hamilton, *Med. Comment.* vol. xvi. p. 317. Cleghorn, *Case, Med. Comment.* vol. ii. p. 229.

‡ Bartholinus, *Burns' Note*, 8th Ed. p. 522.

§ Ruysch, *Obs.* 26.

|| *Journal de Med.* *Burns' Note*, p. 521.

and in the sixth, after the birth of the child, “a discharge of blood now occurred, but it was considerably less than what happened in two of her former labours.” In numerous cases on record the sanguineous discharge is stated, most distinctly, to have been trifling.* In others hemorrhage is not even alluded to as an immediate effect; a strong presumption, that an occurrence which always claims the active energies of the practitioner, could not have existed.† We find in Dr. Hamilton’s observations on his case, that he decidedly states that hemorrhage did not take place, and also that he had seen five cases in which this symptom was altogether absent.‡

As corroborative of the writer’s view, it may be well to state, that in many of those instances, in which it is mentioned that hemorrhage was present, there is the strongest ground to infer that it was not alarming,§ although in some cases, rude treatment of the uterus or placenta had been adopted. In the case related by Mr. C. White, the discharge could not be serious, as the obstetrician, after separating the placenta, and making an attempt to reduce the uterus, left the patient, to wait upon that celebrated practitioner, who did not see the case for an hour afterwards.||

Dr. Davis, in his additional statement to Mr. Newnham’s case, mentions that the hemorrhage was more than ordinary, and that it was observed “to gush out from the passage with much force,

* Brown, *Annals of Med.* vol. ii. p. 278. Brown, *Memoir of Med. Soc.* vol. v. p. 203. White, *Med. Comment.*, vol. ii. p. 268. Dr. Albers, *Annals of Med.*, vol. v. p. 392. Chapman, *Treatise*, p. 123. Welsh, *Med. and Phys. Jour.*, vol. v. p. 451. Smith, *Ditto*, vol. vi. p. 504. Löffler, *Ditto*, vol. xi. p. 208. Harvie, *Pract. Directions*, p. 21.

† Cawley, *London Med. Jour.*, vol. vi. p. 367. Mauriceau, tome ii. p. 559. La Motte, p. 496. Smellie, *Case 3rd, Coll.* 44. White, *Two Cases*, p. 435 and 436. *Treatise*, &c. Edit. 5.

‡ *Med. Comment.* vol. xvi. p. 316.

§ Hunter, *Annals of Med.* vol. iv. Dyson, *Mem. Med. Society*, vol. vi. Clarke, *Edin. Med. and Surg. Jour.* vol. ii.

|| *Treatise*, &c. p. 431.

and as if the source of it had been much nearer to the outlet, than what is common on such occasions. The manner of its escape resembled the spurting from the vessels of a stump during the relaxation of the tourniquet, in the operation of amputation." From this quotation we might infer, that the discharge was both copious and rapid, but we are further told, that "about a pint of blood was lost, *perhaps something more*."* Now every obstetrician knows that this quantity is often lost in common labour.

Hæmorrhage rarely occurs as a primary symptom, but if published cases are consulted, it will be found to happen accidentally, or in consequence of injudicious treatment and violence. In the cases related by early writers on midwifery, we find it stated that the accident was frequently fatal, but at that period the practice was in the hands of ignorant midwives. In the present day a different result takes place.

It has been stated by authors that convulsions frequently occur in this accident, but the cases detailed by the writer warrant him in concluding differently. It was long thought that wounds of the uterus were assuredly mortal, and that they were always accompanied by convulsions. This opinion gave rise to the impression, that accidents, or derangements of this organ, would be attended by this formidable symptom. But greater experience has proved that convulsions are not essentially an accompaniment of wounds of the uterus.† The same unerring guide teaches us that convulsion rarely occurs in inversion, unless the womb has sustained some serious injury from violence, and the same result would frequently be produced in organs less important than this, if the same mischief was inflicted. Those convulsive agitations which are precursory of dissolution, are not the same with the convulsions referred to in the above remarks.

It has frequently happened that death has suddenly taken

* Newham's Essay, p. 35.

† See Hull on the Cæsarean Operation.

place, when neither hemorrhage nor convulsion existed. The event is to be ascribed to that dreadful state of syncope which so generally attends this accident.* This most constant symptom depends upon the strong sympathy existing between the nervous and uterine system; and upon the deficient supply of blood to the brain, and other important organs, in consequence of the accumulation in the vascular system of the abdomen, and produced by the sudden removal of that pressure which all the abdominal viscera had previously received. The pulse is quick and feeble, in consequence of these sudden changes, and it is worthy of remark, that it acquires power as soon as the uterus is reduced.†

In this accident, the patient's appearance is such, as to lead to the conclusion that she must have lost a large quantity of blood; when, in reality, the quick feeble pulse and deliquium, arise from the shock sustained by the nervous and vascular systems, in consequence of the displacement of so important a viscus, which, a short period before, contained the child, placenta, and liquor amnii, and occupied so large a proportion of the abdominal cavity. Some delicate females do not bear the changes which occur in natural labour, without suffering from the effects of syncope.‡ Practical obstetricians are so well acquainted with this circumstance, that position and a regulating bandage are constantly adopted to guard against such a result. Mr. White urged the necessity of not accelerating the passage of the shoulders,§ and Dr. Osborne advised the practice of retarding the exit of the head in order to prevent these consequences. In ascites, the withdrawal of the water without regard to position and abdominal support may induce a fatal syncope.

Although the fainting which happens in inversion is of so serious a nature, yet it is a great satisfaction to know, that if it

* Vide authors already quoted.

† White, Treatise, p. 433.

‡ Ramsbotham, Pract. Obs. part 1, p. 206. Dr. Davis, Obstetric Medicine.

§ Treatise, &c. p. 364.

does not soon prove fatal, there is not much occasion to apprehend danger from its future occurrence. When the first shock of this accident is overcome, and the equilibrium of the circulation restored, other dangers then threaten the unfortunate patient.

When the uterus is inverted, only in a slight degree, the reduction may be accomplished with great ease, and the attempt should be made as soon as it is discovered. As the fundus uteri has not, or only slightly passed through the os, the placenta cannot wholly protrude through this orifice, and, consequently, the fundus should be returned before the placenta is separated.* For if an attempt were made to detach the placenta, the operation must be slow, uncertain, and incomplete, and the danger of hemorrhage incurred, or a greater degree of inversion produced. When the hand is introduced through the os uteri, the fingers should be slightly bent, so as to form a kind of crutch, to carry up the fundus, which sometimes rapidly springs up.† The placenta is now to be separated, and the hand retained until the uterus contracts. In the higher degrees of inversion, the uterus in general remains only a short time unaffected. The os, irritated by the body protruding through it, begins to contract, and soon closely embraces it, and becomes ere long rigid and unyielding.‡ The fundus soon tumifies in consequence of its circulation being interrupted, by the constricting os uteri, and likewise from the unfavourable position in which it lies;§ it is not long before this part becomes tender and inflamed.||

The os uteri is more or less irritable in different indivi-

* See Case 5th of this paper.

† Cases 2nd and 5th of this paper. Merriman, Cases 1st, 2nd. No. 30, Appendix.

‡ See Case 3rd. White, Med. Comment., p. 250. Dr. Denman, Introd. Waller's Ed., p. 420. Dr. Hamilton, Med. Comment., vol. xvi. p. 318.

§ Mauriceau, Obs. Heister, vol. x. p. 1087. Newnham's Essay. *Peu Pratique des Accouchemens*, p. 121. Ruysch, Obs. 10. C. White, Treatise, p. 432.

|| Case 3rd.

duals, and consequently does not always become an obstacle to reduction equally, and at the same time.*

In cases where the accident increases in degree after a certain time has elapsed ; or where it becomes irreducible, the patient suffering but little pain, which occasionally happens ; the os uteri will be found comparatively soft, and not constricting the body in an equal degree. But when the fundus enlarges and inflames soon after the occurrence, then the os uteri is found rigid, and firmly embracing the part.

It is of great practical importance to know that the fundus is so frequently found enlarged, and that the os uteri soon contracts and acquires a rigid character. In one or two hours these changes may have taken place. Dr. Denman was unable to reduce the organ after the lapse of four hours.† When the os uteri is found soft and yielding, it is in a state favourable for the operation of re-inverting the fundus ;‡ but if it is rigid, and girds the body firmly, the fundus being enlarged and tender, then the reduction will always be difficult if not impossible.§

In the treatment of this accident, the great object to be constantly kept in view, is to attempt the re-inversion as soon as possible after the occurrence. But in general the placenta adheres to the inverted organ, and the question is, whether it should be separated or not, before, or after the reduction. It is an important point to settle, especially as there is such a difference of opinion upon the subject. Dr. Denman leaves too much to the judgment of the young practitioner. Dr. Burns advises the fundus to be returned, with the placenta adhering. Dr. Merriman says, “in a case of this kind, which occurred in my practice, the placenta was removed without prejudice, but I think I should not, in future, remove the attached placenta, till after the uterus was restored to its right position.”|| For why ?

* Cases 3rd and 6th Ingleby Tracts and Cases, &c., pp. 226, 227. Dickenson, Med. Gazette, No. 372. Dr. Belcombe, Ditto, vol. vii. p. 783.

† Introduction, &c. Waller's Ed., p. 420.

‡ Case 6th.

§ Case 3rd.

|| Synopsis, p. 151.

The dread of hemorrhage is the reason assigned, why the placenta should not be first detached, but the writer trusts, that the cases he has adduced, and the references he has made, are sufficient evidence to the contrary. In no case has this dreaded effect been induced, or even aggravated by a *complete* separation of the placenta. The uterine vessels are as effectually constricted under this accident as when the organ is in its natural situation, if the placenta be entirely detached; and flooding is produced here, in the same manner as in ordinary cases, by a partial separation or disruption. As the greatest disadvantage arises from failing in our first attempt, it is the more necessary that every impediment should be removed, so that we can proceed with the greatest chance of success. By delay the organ becomes less fit to bear the operation, not only from the increased size of the fundus, and the contraction of the os, but also from the increased sensibility, and irritability, which it has acquired, even previously to its becoming actually inflamed.* The attached placenta must increase the obstacle, because the fundus cannot be so freely and sufficiently compressed. The result of free manipulation would lead to partial detachment and disruption, and consequently to flooding.

By detaching the placenta great advantages are gained; the bulk of the part is diminished, the operator is enabled further to reduce the size of the fundus itself by compression; and he has more freedom to judge of the changes he has effected. If these advantages are denied and the plan be objected to, what method must be adopted, in case we should fail in our endeavours to reduce the uterus whilst the placenta still adheres to it? Surely no obstetrician would, in such a case, think of leaving his patient in that state, until the placenta was detached by the efforts of nature.

But if he then determined to remove this mass, he would have to run the risk of this dreaded hemorrhage, not lessened

* See Case 3rd. Hunter, White, Windsor.

by his interference. When the placenta is detached, our next object should be, to attempt the reduction of the general bulk of the tumour by compressing it.* We are indebted to Mr. C. White for this method. The plan recommended by some writers,† to push the fundus directly upwards should not be adopted. There are strong reasons to think that the fundus is, after the os uteri, the most irritable part of this organ. When the accident has existed a short time, pressure upon this portion induces pain, bearing down, and hemorrhage, but the body may be taken hold of and compressed.‡ If we could press the fundus upwards, and thereby dimple it within itself, we should find ourselves opposed by a double inflexion, for the body would be grasped by the os uteri, and the fundus would be within the body. It is obvious that our force should be directed so as to act upon the angle of inflexion, or where it turns into itself. The plan to be further adopted is stated in the cases related in this paper. Every means should be used to insure the full contraction of the uterus after reduction, and writers dwell upon this point because a second accident has happened.

Dr. Denman's want of success, in not having been able to re-invert the uterus, after it had been down four hours, has tended to paralyze the energies of the obstetrician when he has been called to the case at a late period. If the immediate effects of the shock sustained in this accident are overcome, and the organ remains unreduced, either from inability or want of determination on the part of the practitioner, a number of inconveniences and dangers are to be expected. On account of the dependent position of the tumour bandages and supports are always required, and the active exertions of the patient are greatly impeded. It falls down upon the slightest straining, and influences the functions of the adjacent organs. The uterus itself is liable to inflammation, ulceration, and gangrene, life is miserable, and

* See Case 4th.

† Burns.

‡ Hunter's *Annals of Med.* vol. iv. p. 208.

the patient, sooner or later, falls a sacrifice to the muco-purulent and sanguineous discharges.

These circumstances, with others, relating to the married state, demand and justify the removal of the uterus when irreducible. But it is the opinion of the writer, that this operation, as before stated, will not be often required, if a proper view is taken of the circumstances which impede the reduction. The energies of the obstetrician should not give way, if unsuccessful in his first trial, but he should repeat the attempt. When the inflammatory symptoms come on, they are to be combated by active remedies, and when subdued further exertions should be made to re-invert the organ.* In some of the cases which are detailed in periodicals, we find no attempt made after the first has failed. The inverted womb has, however, been re-inverted after a considerable length of time,—of six or seven hours,† of seventeen hours,‡ of twenty-four hours,§ of three days,|| of twenty-seven hours,¶ of seven days,** of eight days,†† and in one case after it had existed twelve weeks.‡‡

ART. XII.—*Remarks upon certain Injuries of the upper Extremity of the Humerus.* By ROBERT W. SMITH, A.M., M.R.I.A., Lecturer on Surgery at the Richmond Hospital School of Medicine, &c. &c.

WHEN we consider the varied and extensive motions of the shoulder joint, and their importance in the daily occupations of life: when we reflect upon the liability of this articulation to accidents, and the difficulties which sometimes present themselves to the surgeon, when he is seeking to distinguish between the luxations and the fractures of its bones, and upon the still

* See Case 3rd.

† Löffler's Case.

‡ White's Case.

§ Mr. Wynter's Case.

|| Mr. Cawley's Case.

¶ Mr. Dickenson's Case.

** Case 6th.

†† Mr. Ingleby's Case.

‡‡ Dr. Belcombe's Case.

more embarrassing nature of the case when these two lesions co-exist, we must at once see and admit the necessity of investigating with the utmost patience, a class of injuries, which, if neglected or misunderstood, entail great misery upon the patient. Much, certainly, has been done already to clear away our doubts upon these injuries by Boyer, Sir A. Cooper, Dupuytren, Malgaigne, and others, but something still remains to be accomplished, for in every hospital, cases are frequently seen, which almost defy the ingenuity and discernment of the experienced surgeon to explain, and the true nature of which, dissection has not yet revealed. Dupuytren himself has said, and said truly, that all solutions of continuity of bones, in the neighbourhood of joints, are the sources of numerous errors. It is to the rarer and more obscure injuries of the bones of the shoulder joint, that I wish to draw attention, and trust that I shall be able in the following observations to remove some of the doubts and uncertainties by which they are enveloped.

CASE I.—In July, 1834, I was called upon to examine the body of Julia Darby, æt. 80, who had died of chronic pulmonary disease. Upon entering the room, the appearances of the left shoulder joint at once attracted my attention, and struck me as being different from those which attend the more common injuries of the joint. The shoulder had lost, to a certain extent, its natural rounded form; the acromion process, though unnaturally prominent, did not project as much as in any of the luxations of the head of the humerus. The breadth of the joint was doubled; upon pressing beneath the acromion, I could plainly distinguish a portion of the head of the bone occupying the inner point of the glenoid cavity; it formed a tumour, perceptible through the soft parts, while the remainder, and by far the larger portion of the head of the bone, lay beneath the level, and internal to the coracoid process; and between these two portions, the finger sunk into a deep depression or sulcus, placed immediately below the coracoid process. The elbow could be brought into contact with the side, and there was no appreciable

change in the length of the arm. Such were the external characters of the injury, and from these alone I was unable to pronounce positively as to its exact nature, but conjectured that it was some variety of the luxation forwards. Upon removing the soft parts, the head of the bone presented itself, increased to nearly double its natural breadth; it lay beneath, and internal to the coracoid process.* The greater tubercle was completely broken off from the shaft of the humerus, and in situation corresponded to the inner part of the glenoid cavity; the fracture traversed the bicipital groove, which, in consequence of the displacement which the head of the bone had suffered, was situated exactly below the coracoid process; the glenoid cavity was changed both in form and size, it was smaller than natural, nearly flat, and broader above than below. A new, shallow socket was formed for the head of the bone, upon the axillary margin of the scapula, and bony matter was deposited in the capsule, which was greatly enlarged; the cartilage had been nearly altogether removed from the head of the bone, which was covered by an ivory deposit. The injury was evidently of very long duration, but of the exact time and mode of its occurrence I could obtain no precise information.

In November, 1833, a lecture was delivered by Mr. Guthrie, on a peculiar injury of the shoulder joint; this lecture (which was published in the *London Medical and Surgical Journal*, and also in the *Lancet*) I had not read, when the case above detailed came under my observation. But the appearances of the injury in the cases detailed by Mr. Guthrie, are so very similar to those now described, that I am pretty sure the lesion was also similar. The first case mentioned by Mr. Guthrie, is that of Louisa Chapman, æt. 11; she had fallen upon the shoulder; a considerable prominence was noticed upon the inner of the anterior portion of the joint, a prominence liable to

* See plate.

be mistaken for the displaced head of the bone, were it not that the whole of it did not project internally, but on the contrary, the greater part could be felt in the glenoid cavity.

“If,” says Mr. Guthrie, “the thumb or fore-finger of the left hand be placed upon the internal protuberance of bone, whilst the elbow is rotated by the right, the protuberant portion of the humerus is found to obey the motion given to the elbow, in the clearest possible manner. If the fore-finger be carried outwards and placed upon that part of the joint in which the external part of the head of the humerus ought to be placed in its normal state, it will be found there, and moving most distinctly under the finger when the elbow is rotated; the arm can be elevated and the hand placed on the top of the head; there is no peculiar or decisive sensation communicated to the finger when pressed into the axilla, and the elbow can be pressed close to the side, although it tends a little backwards; if the point of the fore-finger be placed immediately below the middle of the acromion process, it sinks into a hollow between the protuberant point of bone, and the articulating part of the humerus which moves in the glenoid cavity, and if the two shoulders are compared, the greater breadth of the injured one is apparent. What is the nature of the injury? dissection has not explained it, but I believe that it is a longitudinal split of the humerus; the accident always happens in consequence of a fall on the point of the shoulder, in which the head of the humerus first meets the ground, and receives the shock. I believe the split separates the small tuberosity, with more or less of the head, and extends in the direction of the bicipital groove, and I suspect that the tendon of the pectoralis major in front, and those of the latissimus dorsi and teres major behind, prevent displacement by acting in the manner of a hinge.” Mr. Guthrie adduces three other instances of the accident, in all of which the symptoms were nearly alike; in the third case, which is that of Colonel Yorke, who fell from his horse, on his left shoulder, the only symptoms differing from those in the case of Chapman, were a

little more protuberance on the inside, and a crepitus on rotating the elbow. The fourth case is that of a gentleman, aged eighteen, who fell upon the point of the shoulder, and was supposed to have dislocated the head of the humerus; the bone, when reduced, did not, however, remain in its situation. At the end of three weeks, Mr. Guthrie first saw the patient; he found that the elbow was carried very far backwards, and the head of the os humeri proportionably forwards, and apparently dislocated in that position, but the prominence was not round, like that of the head of the bone, and did not give that sensation to the touch; on the contrary it felt as if the head of the bone was split as well as dislocated. In the attempts which were afterwards made to reduce the bone, the prominence could not be entirely removed; the outer portion of the head of the humerus appeared to be in the glenoid cavity, but the greater part of it was internal to it. In this case Mr. Guthrie supposes that the capsular ligament of the joint must have been torn, and the whole head of the bone dislocated as well as split, which might be a reason why it so readily became displaced a second, and even a third time.

Before making any observations upon these cases, or upon that, the dissection of which came under my own notice, I shall detail the particulars of one or two other cases which were lately admitted into the Richmond Hospital.

CASE II.—John Morgan, æt. 62, on the 1st of January, 1836, while running smartly tripped over a stone, and fell with violence upon the left shoulder. When the man was admitted into hospital, which was several months after the occurrence of the accident, the injured limb presented the following appearances: the acromion was unnaturally prominent, but the finger could not be sunk into the glenoid cavity, at the inner part of which, beneath the coracoid process, a larger, prominent, bony tumour could be felt; and externally, beneath the acromion, a second osseous prominence was perceptible even to the eye; these two projections were separated by a deep and distinct vertical depression, corresponding to the line of the bicipital groove;





there was no appreciable alteration in the length of the arm, or in the depth of the anterior wall of the axilla; the breadth of the joint was nearly double that of the opposite one, the head of the bone was changed in form, and irregular, and the direction of the axis of the humerus altered; the muscles upon the dorsum of the scapula were wasted, the arm was powerless, the elbow stiff, and the wrists and fingers permanently extended; the scapula moved with the arm. The opinion which I formed of the case was, that there had been a fracture, which traversing the upper part of the bicipital groove had detached the greater tubercle of the humerus, and that, as a consequence, the head of the bone was slightly displaced forwards; this was also the opinion of Mr. Adams, under whose care the patient was placed. As the man still lives any opinion upon the case must, of course, be cautiously received; although from the resemblance which the symptoms bear to those noticed in the case of Darby, there is every probability that the nature of the injury was similar in the two cases.

CASE III. —A man aged about 30 years, fell from a house three stories high, and received a severe injury of the left shoulder joint. Upon his being conveyed to the Richmond Hospital, the pupil in attendance, finding the head of the bone placed more forwards than natural, the acromion process more prominent, the deltoid muscle, to a certain degree flattened, the elbow separated from the side, and all motion painful, especially rotation outwards, while the hand was in a state of pronation, conceived that the case was one of dislocation of the head of the humerus forwards, under the pectoral muscle. The mode of reduction by the knee in the axilla was had recourse to, and the arm was bandaged to the side. However, after a short time, the patient not having experienced any relief, the bandage was removed, when it was found that the joint presented exactly the same appearance as at first; and, upon more close examination, a crepitus was distinctly heard; this circumstance, along with the recurrence of the deformity, led to the idea that the case was one of fracture of the neck of the scapula. However, when

the patient was visited by Mr. Adams, the power of diagnosis, with which this intelligent surgeon is gifted, at once enabled him to form a more correct opinion, and he expressed his belief, that there was neither luxation of the humerus, nor injury of the scapula. I saw the patient along with Mr. Adams, and fully concurred with him, as to the nature of the injury. The appearances which the joint presented were similar to those noticed in the cases of Darby and Morgan: these were briefly as follows: the acromion was more prominent than natural, but still the finger could not be sunk into the glenoid cavity; the arm was separated from the side, but could be easily approximated to it; the breadth of the joint was nearly double that of the opposite one, and two tumours were distinctly noticed, the inner one the larger of the two, and placed under the coracoid process, was evidently constituted by the head of the humerus; while the external and smaller was apparently formed by the greater tuberosity, and the two were separated by a deep and well marked sulcus, corresponding to the line of the bicipital groove. I conceive then, that in this case, the injury was a fracture, passing through the upper part of the bicipital groove and detaching the greater tubercle from the shafts of the bone.

I do not see how the appearances are to be explained upon any other supposition; it could not have been a fracture of the neck of the scapula, for in this case the arm is lengthened, and the deformity is readily removed by pushing the elbow upwards, but re-appears when the limb is unsupported; whereas in the case under consideration, there was no lengthening of the arm, on the contrary, it was slightly shortened, nor could the deformity be completely removed by any means whatever. At first sight, the appearances of the joint, no doubt, resembled those of dislocation forwards, but, the facility of approximating the arm to the side, and the great increase in the breadth of the joint, afforded ready marks of distinction. Now, if we compare the appearances in the instances which I have detailed with those described in Mr. Guthrie's cases, it must be apparent,

that the nature of the injury was similar in all, and, by a reference to the post mortem examination, in the case of Darby, I think it must be allowed, that although Mr. Guthrie's conjecture approximated closely to the truth, he has not exactly made out the nature of the lesion. The situation of the fracture may not be, and most probably is not, the same in all cases, yet some part of the greater tubercle is, in my opinion, in every instance its seat, and not the lesser tubercle, with more or less of the head of the bone, as supposed by Mr. Guthrie. I have never ascertained by post mortem examination, that the fracture extended vertically, or obliquely, so far down the shaft of the bone, as to be influenced by the muscles which constitute the folds of the axilla, nor, even though it did do so, can I clearly understand, how, as Mr. Guthrie supposes, the tendons of these muscles could act in the manner of a hinge, so as to prevent displacement. What occurs in the cases under consideration, is, in my opinion, simply this,—a fracture traversing the upper part of the bicipital groove, detaches the greater tubercle of the humerus, thus annulling the action upon the humerus of the supra-spinatus, infra spinatus, and teres minor ; the folds of the axilla, the subscapularis and the anterior portion of the deltoid, then act almost unopposed, and draw the head of the bone forcibly inwards against the inner part of the capsular ligament, and if, at the same time, the inner border of the glenoid cavity be broken, (which, I suspect, is by no means a rare occurrence,) the head of the bone passes still further inwards and beneath the coracoid process, amounting, at length, to an actual displacement, which is permitted by the increased size of the joint, just as a displacement of the head of the femur will often be the consequence of a fracture of the acetabulum.

In the fourth case, detailed by Mr. Guthrie, that of a gentleman, æt. 18 ; it was supposed that the head of the bone was dislocated as well as split ; it became displaced a second, and even a third time, after having been reduced ; and Mr. Guthrie supposes that the capsular ligament must have been torn, and

the whole head of the humerus dislocated ; I think it is more probable, that, in this instance, the glenoid cavity was broken and the greater tubercle detached ; for, in the cases under consideration, the injury is always the result of violence, applied directly to the shoulder joint, and, under such circumstances, we know that fracture is to be expected, rather than luxation, which will much more probably occur, when the violence is applied at a distance from the shoulder, as when a person, in falling, alights upon the elbow. The facility with which the displacement re-appears, after reduction, is also upon this supposition readily accounted for ; just as in a case which came under my observation some time since, in which the neck of the femur became a second and a third time displaced, in consequence of a fracture of the acetabulum.

The symptoms of the injury which I have been attempting to describe, and the degree of displacement which the head of the humerus suffers, must vary considerably, according as the fracture has separated more or less of the greater tubercle : if a small portion only be detached, the abnormal appearances of the joint will be but trivial, and, perhaps, the injury will altogether escape discovery ; but, if the entire of the process be separated, it will give rise to a considerable increase in the breadth of the joint, for the inner portion of the bone will be drawn inwards, and a deep sulcus will be apparent in the site of the bicipital groove ; and lastly, if along with the fracture of the tubercle, there is a second traversing the inner part of the glenoid cavity, there results from this double lesion the greatest degree of displacement which can occur, without laceration of the capsule and actual luxation, and it is in this last case, particularly, that the frequent recurrence of the displacement after reduction is so remarkable.

In a case published by Mr. Crampton,* the attachments of the tendon of the supra-spinatus and infra-spinatus muscles

* Dublin Journal, vol. iii.

were torn off, along with the part of the bone they were inserted into, and the same circumstance was long ago noticed in a case detailed by Mr. Thompson ;* but both these cases were examples of true and complete dislocation, to which it is not my object to allude at present.

It does not seem to me very difficult to distinguish fracture of the greater tubercle, from luxation of the head of the humerus. One of its most remarkable and diagnostic features is the great increase in the breadth of the articulation ; moreover, the glenoid cavity is not entirely abandoned ; the acromion process is not as prominent as in luxation ; the displacement is very readily produced ; we cannot depress the deltoid muscle, as in dislocation ; and lastly, the violence which produced the lesion has been directly applied to the injured part. With respect to the prognosis, I conceive that a fracture, analogous to that found in the case of Julia Darby, must be exceedingly difficult of cure, most probably ligamentous union is the most we can expect. In this respect, the injury somewhat resembles fracture of the neck of the femur within the capsules ; in the latter case, one great obstacle to osseous union is the difficulty of keeping the joint motionless ; in the former, the same cause, although perhaps operating to a less amount, must still, I conceive, to a certain extent interfere with the process of union by bone ; and again, it is not less difficult to keep the fragments in contact, for the head of the bone will be drawn inwards by the folds of the axilla, while the tuberosity will move in the contrary direction, obeying the action of the muscles inserted into it. Thus there is a considerable displacement, according to the breadth or thickness of the bone ; and to this will be added a displacement according to its length, if the head of bone be drawn inwards, sufficiently far to enable it to clear the coracoid process ; for it will then be drawn upwards by the coraco-brachialis, biceps, and other muscles, whose direction is nearly parallel to the axis of the humerus. Such, in

* Med. Obs. and Inq. vol. ii.

fact, was the case in the preparation, from which the accompanying plate has been taken, for the lesser tubercle was placed above the greater ; whereas, in the normal condition of the humerus, the reverse is the case, the summit of the greater being above that of the lesser tuberosity.

Before leaving the subject of displacement of the heads of bones, as a consequence of fractures extending into, or in the neighbourhood of joints, I shall detail the particulars of a case of fracture of the acetabulum, which I lately had an opportunity of seeing, and to which I have already alluded.

Thomas Venables, æt. 25, on the 4th October, 1834, received a severe injury of the right hip-joint, in attempting to leap across a ditch: while running to the leap he stumbled, in consequence of the softness of the ground, and in order to clear the ditch, was obliged to make a very powerful spring ; he alighted with great violence upon the right leg, and in a very oblique manner, the outer side of the foot and heel sustaining the principal shock, which he described to have been terrible. He fell immediately, and was unable to rise from the ground, or to walk or stand when raised. Soon after the occurrence of the accident he was seen by Surgeon Hilles, under whom he remained for some time, and to whom I am indebted for an account of the appearances which the limb presented immediately after the accident. When the patient was supported in the erect posture, the appearance of the limb excited at first sight a suspicion, that the head of the femur was dislocated either upon the dorsum of the ilium, or into the sciatic notch. The limb was shortened about two inches, the heel drawn upwards, the foot was inverted, the knee advanced, and thrown across the opposite one, and a bony tumour was perceptible upon the dorsum of the ilium ; rotation outwards gave great pain ; the trochanter was raised nearly to a level with the anterior superior spine of the ilium, but was one inch further removed from it than natural. No crepitus was discovered. On the following morning, an extending force being applied, the head of the bone

passed into its natural situation, and the deformity of the limb disappeared ; the bone, however, did not pass into its socket with the usual and well known noise ; on the contrary, its gradual change of position was accompanied by a most unpleasant cringing or grating sound, like that produced by the rubbing together of the two fragments of a broken bone ; the patient was placed in bed, and the limb secured in the usual manner.

Monday, 6th.—When the patient was visited this morning, it was found, that during the night the head of the bone had started from the acetabulum, and all the former signs of the injury had re-appeared, and it was observed that, although the foot was inverted, the patient had the power of everting it to a considerable degree. This circumstance first gave rise to the suspicion, that the injury was not a simple dislocation. On Tuesday the displacement was a second time reduced ; and the case proceeded favourably until Friday night, when, owing to the disturbance produced by a purgative medicine, the dislocation recurred a third time. An accurate examination being now instituted, to discover the existence of a fracture, a distinct crepitus was perceived at the upper and posterior part of the acetabulum. The crepitus, the frequent recurrence of the displacement, and the integrity of every portion of the femur, rendered it sufficiently obvious, that the brim of the acetabulum was broken in the above-mentioned situation. The bone was a third time restored to its place, and a strong band passed around the pelvis. In 1836 the man was admitted into the Richmond Hospital, under the care of Mr. Adams, who had seen the case from its commencement. He was at this time unable to walk without the assistance of crutches. The injured limb was one inch and a half shorter than the other ; when standing, he rested it upon the points of the toes, the heel being drawn upwards ; but a slight degree of extension was sufficient to restore it to its natural length ; and, indeed, when the man was lying in his bed, there was scarcely any perceptible difference in the length of the two limbs : the breadth of the injured hip was considerably

greater than that of the sound one, and the head of the femur obviously enjoyed an unnatural extent of motion; in fact it moved in a socket that was much too large for it; it could be pushed upwards, so as to produce a shortening of the limb, to the extent before-mentioned, and it always ascended when the patient endeavoured to support his weight upon it, and in many of the motions of the joint, the rubbing together of the broken surfaces was still distinctly audible; there did not appear to have been any attempt to accomplish osseous union.

The above case presents us with an example of an accident, which is of very rare occurrence, and one which, from its liability to be misunderstood, and the difficulty which attends its treatment, is well worthy of attention. What are the cases with which it may be confounded? they are, I conceive, the ordinary dislocation upon the dorsum of the ilium, or into the sciatic notch; the congenital luxation of the hip, and fracture of the neck of the femur with inversion of the foot. In the case which I have related, the shortening of the limb, the inversion of the foot, and the tumour upon the dorsum of the ilium, were strongly calculated to mislead the judgment, but the power which the patient possessed of everting the foot and the existence of a crepitus, sufficiently distinguished the injury from the ordinary luxation upon the dorsum of the ilium; the crepitus, however, was not at first perceptible, it was heard for the first time during the attempts at reduction; the rough, grating sound then produced was the first circumstance which raised the suspicion that the case was not one of simple luxation, but rather of displacement with fracture, the former the consequence of the latter, and the frequent recurrence of the displacement after reduction shewed the correctness of this opinion. The injury, under consideration, resembles in many respects the congenital luxation of the hip. In the latter, as Dupuytren has remarked, every time that the person steps, the head of the femur rises into the external iliac fossa, and all the symptoms of the displacement become more evident; because, the head of the femur

not being fixed, undergoes a continual displacement, and is elevated or depressed, according as it bears or is free from the weight of the body. When the person lies down, the symptoms of the displacement, in a great degree, disappear, and in this position we can shorten or elongate the limb at pleasure by a slight extension, or by pressing it upwards; or if we grasp the iliac crest and great trochanter, it becomes evident that the bone suffers a displacement to a variable extent, and by this elongation and shortening, the signs of the displacement appear and disappear.

The same or similar symptoms occur, when, the brim of the acetabulum being broken, a partial displacement of the head of the femur is produced; the limb can be shortened or elongated by extension or by pressing it upwards, and the rising of the bone into the iliac fossa always occurs, when the weight of the body is thrown upon the injured limb, for the acetabulum is increased in size to a degree corresponding to the extent of the fracture; the capsular ligament is powerfully stretched over the head of the bone, and in time acquires a great degree of thickness; in some cases it is torn, and the ligamentum teres, if not ruptured, acquires, in all probability, a gradual increase of length.

We see, therefore, that the two cases resemble each other, in the circumstances of our being able to elongate or shorten the limb by extension or by pressing it upwards, and the ascent of the bone when the weight of the body is borne upon the affected limb; in both cases, also, the foot can be more or less everted; notwithstanding, however, this close similarity, the diagnosis is simple, for the very circumstance of the one being a congenital defect, is enough to distinguish between them. The recurrence of the displacement, and consequent shortening of the limb, when the extending force ceases to act, might perhaps induce some to look upon the case as one of fracture of the neck of the femur, with inversion of the foot; but we have so many diagnostic marks between these accidents, that such an error would be almost inexcusable; it is, no doubt,

true that in both cases the patient can evert the foot to a certain degree, and in both there is crepitus; but in the former case, when we rotate the limb, the integrity of every portion of the femur is manifest, the trochanter moves in the segment of a much larger circle than it does when the neck of the femur is broken, and the shortening of the limb, after the extending force is withdrawn, occurs at a much later period, and may perhaps, by proper treatment, be altogether prevented.

Mr. Scott has published a case,* in which the upper part of the brim of the acetabulum was broken, and the head of the femur displaced upon the dorsum of the ilium, the limb was shortened two inches, and the toes turned inwards, and it was remarkable that the motion of rotation outwards, though painful and difficult, could, to a certain extent, be accomplished.

I think, therefore, that we may state the diagnostic signs of this injury to be the crepitus, the degree of motion being greater than in luxation, the possibility of everting the foot, the recurrence of the shortening some time after the extending force has ceased, and the consequent re-appearance of the original symptom; it is an injury exceedingly difficult to treat, and very liable to be succeeded by permanent lameness.

ART. XIII.—*Case of Hydatid Tumour*. By JOHN J. RUSSELL, Assistant Surgeon, 73rd Regiment.

IN the month of May, 1833, the following case was admitted into the Hospital of the 63rd Regiment, then stationed at Hobart Town. It was seen by several surgeons of the place as well as by many naval surgeons, who occasionally visited the port. Its nature and exact situation were, prior to the operation, mere matter of conjecture to us. There is a case recorded of much resemblance to it in the *Medico-Chirurgical Review* of January, 1828.

* Dublin Hospital Reports, vol. iii.

May 18th. George Aram, 63rd Regiment, master tailor, æt. 36, a pale, unhealthy looking man, prone to excessive indulgence in liquor, admitted, complaining of an obtuse pain or uneasy sensation in the epigastric region, somewhat increased by pressure. On close examination, there can be indistinctly felt a tumour projecting from under the ribs of the right side of the thorax, extending to about two inches to the left of the ensiform cartilage, and descending a little below the umbilicus, apparently as if it were an enlargement of the left lobe of the liver. The affection, he states, has been coming on for several months, but not giving pain, he did not attend to it. The action of the bowels is very irregular; evacuations sometimes scanty, pale, or slimy, at others confined for two or three days together; countenance sallow, exhibiting in appearance that of a man affected with chronic hepatitis; pulse weak, and but little accelerated; appetite capricious; he frequently has a sense of fulness in his stomach.

A cathartic of calomel and rhubarb was given, and cloths, saturated with a lotion of nitro-muriatic acid, were frequently applied over the tumour.

30th. Within the last three days, the *lower* part of the abdomen has become enlarged, and now a fluctuation is obscurely felt, bowels are open, stools cream-coloured; no apparent alteration in the tumour itself.

Persistat in usu Solut. Acid. Nit. Muriat.

R Subm. Hyd. gr. ii.

Pulv. Scillæ gr. iii.

Digit. gr. i.

M. ter in die sum.

June 3rd. The tumour increases, extending an inch below the umbilicus; on the right side it becomes less distinct, but more prominent in the centre; the distention in the lower part of the abdomen subsides; evacuations more natural; pulse regular.

Omitt. Solut. Acid. affricetur tumori Ung. Hyd. Camphor. mane nocteque. Cont. Pil.

6th. No apparent alteration in the tumour ; gums affected by the mercury ; bowels regular.

Omitt. Prepar. Hydrarg. Affricetur pars affect. Liniment. Stimul.

9th. Tumour stationary ; the effusion into the lower part of the abdomen altogether absorbed ; he becomes more weakly and emaciated ; appetite continues good, pulse natural, tongue clean, sleep much disturbed, bowels regular, stools again cream-coloured.

Sumat Confect. Opiat. 3ss. h. s. cont. linim.

15th. The colonial surgeon, and several other medical men, were invited to see this case by Surgeon Bohan and me, that we might have the advantage of their opinions, but they were unable to come to a positive conclusion as to its composition or precise situation, some considering it to have its seat in the omentum, and others in the liver, or on its inferior surface. I am inclined to the latter opinion. It has assumed a circular form, extending from the scrobiculus cordis to about *two* inches below the umbilicus, the exact border is undefined ; pressure on the centre communicates an indistinct sensation of a fluid.

Applicr. Cataplasma quater in die. Cont. haust. sed. h. s.

17th. No change in appearance of tumour. Complains of being very restless at night. Bowels confined, tongue foul.

Sumat. Haust. Purg. statim.

R Liq. Opii sed. M. xxv. h. s.

21st. Cataplasma produces no sensible effect.

July 6th. The tumour becomes more circumscribed generally, it is about the form of an inverted saucer ; a portion of its inferior margin is felt like a firm ridge, extending transversely from two inches below the umbilicus, towards the anterior superior spinous process of the right ilium. Its centre gives a sense of

fluctuation rather more distinct, with some *elasticity*. Bowels inclined to constipation.

Rep. Haust. Aperiens. Cont. Haust. sed h. s.

14th. The swelling is now much increased and more conical; its centre projects, and gives a sense of fluctuation; the limits of its circumference are the same, but the integuments are tense and glossy, bowels inactive, countenance of a sickly pallid hue, considerable debility, nights restless, patient's mind anxious, and he appears to participate in the general curiosity to ascertain the contents of the enlargement. Being almost certain that *some* fluid was contained in it, and from the anxiety of the patient, I determined to open it. In the presence of Messrs. Bohan, Scott, and Seccomb, I made an incision *two* inches long, in a straight line midway between the umbilicus and ensiform cartilages, which point was the apex of the conical projection through the integuments and through a thin cyst, on the opening of which an immense *gush* of perfectly formed *hydatids* took place, and continued, with the assistance of gentle pressure, to pass out for a considerable time. They were of various sizes, some nearly as large as a goose's egg, down to the size of the smallest pea. Many were broken in passing out. The quantity discharged (being counted by the Hospital Sergeant till he was *tired*) amounted to nearly *two thousand*; and measured, with the fluid, which was mostly the contents of the broken hydatids, a *gallon* and a half. They were spherical, heavier than water, their fluid like the white of egg, but not coagulable by heat. A piece of lint introduced, and light dressing and bandage:—

Sumat Vini Rubri 3x.

Habt. Haust. sed. h. s.

During the time of the escape of the hydatids, and after the first alarm was over, it was amusing to observe the patient's surprise, who innocently inquired, "be they eggs, Sir?" I introduced two fingers to clear the passage, which was obstructed

by the gush, and distinctly felt the projection of the *spine* with something which I took for the *pancreas* at the back of the cavity.

July 19th. This morning fourteen more hydatids came away with the dressings; their coats appeared more thick and less translucent than the first. Sleeps better, appetite pretty good, bowels regular, mind more tranquil :

Continue anodyne, wine, and pudding.

22nd. Twelve more hydatids escaped, their surfaces covered with a *yellow*, viscid matter resembling *bile*.

Cont. med.

26th. Six more escaped, so yellow as to appear like yolks of eggs. Debility.

Cont. omnia Med., &c.

27th. Ditto.

Sumat. Quinæ Sulph. gr. iii. ter in die.

August 18th. *Feculent* matter of an *intolerably offensive* smell has come away daily for the last fortnight, with an occasional hydatid or bit of membrane, and this morning, for the first time, about three drams of *pus* escaped.

22nd. The discharge which comes away *daily* now, is at first of a serous description, it then becomes purulent, mixed with dark-coloured viscid matter, and amounts to a *pint*; the odour is very offensive, but no purulent matter has appeared for the last two days. The patient's general health improves, he now occasionally walks about the ward, appetite continues good, bowels regular :

Cont. omnia.

31st. General health improving; discharge is about twelve ounces daily of *pus* and serum.

September 20th. Discharge about *half* a pint daily; becoming less and less offensive; health improves.

November 1st The discharge has come down to about two

ounces at each dressing ; improved in all respects, but retains his pallid countenance, with a slightly œdematous appearance.

December 13th. The dressings require changing daily, but the discharge is very slight. The patient walks about in the open air ; his spirits and appetite are good, and he may be considered convalescent. This man being unfit for active service has been discharged with a pension to remain in the colony. I heard lately that he was well and enjoying tolerably good health, *three years* after the operation for his relief.

ART. XIV.—*On Aortitis, as one of the Causes of Angina Pectoris ; with Observations on its Nature and Treatment.* By D. J. CORRIGAN, M. D., Lecturer on the Theory and Practice of Medicine, at the Dublin School of Anatomy, Surgery, and Medicine, Physician to Jervis-street Hospital, Dublin, Consulting Physician to the Royal College of St. Patrick, Maynooth, &c.

I HAVE two objects in view in these observations, 1st, to shew that *inflammation of the lining membrane of the mouth of the aorta* is capable of producing the group of symptoms to which we give the name of Angina Pectoris, and is therefore entitled to a place in the list of the causes of that formidable affection ; 2ndly, to trace the pathology and treatment of aortitis.

I shall relate the cases in the order in which I believe they will throw most light upon the disease, first giving the cases in which the patient died, while the disease was acute or recent, then those cases of longer duration, which exhibit the alterations of structure produced by the disease, when uncontrolled in its progress, and lastly, those cases in which, from analogy with the symptoms of the former cases, there was every reason to suppose the existence of the disease, and in which, treatment founded on the supposition was attended with success.

Of the first class of cases, or those exhibiting the pathology of disease in its recent or acute stage, there is a good instance

in Portal's *Anatomic Medicale*, vol. iii. p. 127. A young man, ill of small-pox, died of the disease suddenly receding. The symptoms were violent suffocations, accompanied with palpitation. The aorta, through its whole extent, was very red, its parietes swelled and soft, particularly in the thoracic region, where the artery was covered with varicose vessels, and its internal membrane was swollen and softened.

The second is from Bouillaud's recent work on *Diseases of the Heart*, Appendix, p. 372. A young man, ill of typhoid enteritis, was seized with great agitation, accompanied with difficulty of respiration, and disturbance of the circulation, and died suddenly with these symptoms, such as are not usually met with in fever. On dissection, the aortic valves were found covered with a thin granular false membrane. This fibrinous membrane could be torn off like that found in pericarditis or pleuritis. The layer in contact with the valve seemed already organized, and there can be scarcely a doubt, that in some days it would have become adherent to the valves, and would have produced thickening. On the surface of the valves, as well as on the lining membrane of the aorta, there were red points, and below the valves the lining membrane of the ventricle was opaque and milky.

The following cases came within my own observation.

CASE I.—— Glynn, ætat. 39, carpenter, was admitted into Jervis-street Hospital on the 19th June, 1837. He was under the care of my friend and colleague, Dr. Hunt, to whom I am indebted for this case. He had been complaining for three months previously of debility and cough, accompanied with attacks of dyspnoea, attacking him when walking or working, and obliging him to stop frequently to sit down. He also suffered acutely from sensations as if of tearing asunder in his chest.* After exercise he suffered from palpitation.

* "Il semble au malade que des ongles de fer ou la griffe d'un animal lui décharent la partie antérieure de la poitrine."—LALANDE.



Physical examination of the chest gave extensive dulness over the præcordial region, with tumultuous and indistinct action of the heart, and puerile respiration in the lungs, but no other sign of disease. On the 24th, five days after admission, head symptoms set in, viz., dry retching, followed by delirium and stupor, and he died on the 26th.

P. M. There was some fluid in the pericardium ; the whole heart was enlarged, the left ventricle and auricle particularly so. The lining membrane of the aorta, just above the valves, was of a vivid red colour, and was protruded considerably beyond the natural level by an effusion of red, and (apparently) organized lymph, which lay behind it, effused between it and the fibrous coat of the vessel. This vividly red and swollen portion of the vessel contrasted strongly with the pale and polished surface of the artery a little further on.

CASE II.—The case of the late Mr. T., an apothecary, furnished me with the next instance of the disease. I saw him in May, 1837, when he walked into my study to consult me ; the dyspnœa was most distressing, and painful in the extreme even to look at ; his face was pale, and expressive of the greatest anxiety ; his pulse quick and small, and he altogether presented a distressing picture of intense Angina Pectoris. He came, he said, to seek for relief from paroxysms of dyspnœa, which were accompanied with feelings of great oppression across the chest, and sensation of impending death. These paroxysms commenced about five or six months ago, and had latterly become very frequent. He is seized by them when walking in the street, when stooping to tie his shoes, even when lying in bed. If seized by them when moving, he is obliged to hold by any thing within reach, and to abstain from any, even the slightest attempt at any further exertion. He pants, and dare not lie down, if in bed, but is obliged to remain stooped forward as long as the paroxysm lasts.* On examining the ac-

* The symptoms of a paroxysm in this case coincide accurately with Dr. Forbes's description of a paroxysm of angina, given in his admirable article on

tion of the heart, I found it strong, with intense bruit de soufflet under the first bone of the sternum, and below the left mamma. When the paroxysm was not on him, the pulse was full, and about 100. In this instance, assisted by the cases I had previously seen, I made the diagnosis for the first time, and referred the paroxysms to inflammation of the lining membrane of the mouth of the Aorta; I treated him on this supposition. Leeches were applied over the region of the heart, followed by blistering; and blue pill was given in small doses, until the mouth was made sore. To arrest the paroxysms, which it was generally successful in doing, a draught was given of 3ss. of laurel water, with fifteen drops of *Tra opii*, and three or four grains of camphor. As soon as the mouth was made sore, he was put on *digitalis*. Under this treatment the amendment was rapid. At the end of a fortnight the fits of dyspnœa had ceased, the pulse fell to seventy; and, to use his own expression, he felt that he was quite well. The bruit de soufflet, though much less intense, continued quite audible. Just, however, at this time, when all seemed so

the disease, in the *Cyclopædia of Practical Medicine*. "Whilst walking or running, and more particularly if in the act of ascending an elevation at the same time, he is all at once seized with a most distressing sensation in the chest, usually in the region of the heart, or about the lower half of the sternum, and towards the left side. This sensation is variously described; as pressure, or stricture, or weight, or as a positive pain of various character and degree, sometimes obtuse, some acute, tearing, burning, or lancinating. There seems always to be something peculiar in the pain, whatever be its degree, unlike the pain of other parts of the body, and, as it were, combined with something of a mental quality. There is a feeling, a fear of impending death, and the primary symptoms of corporeal disorder are speedily modified by the consequences of mental impressions, conveyed through the nervous system. The patient makes a sudden pause, eagerly catches hold of whatever is next him for support, perhaps raises the hand to some object above him, to which he clings, or it may be, imperceptibly sinks down on a chair or bank, as if unable to stand, yet afraid of the movements necessary to seat him. The face is pale, the expression of the countenance haggard, the whole body is covered with a cold sweat, and death appears to be impending, no less to the inexperienced spectator, than to the miserable patient."

favourable, the change was awfully sudden. He had gone to bed as well as usual, when after having been lying but for a short time, he suddenly sprung out of bed, exclaimed that he was gone, fell back, and expired. I examined the body next day. The lungs were healthy; the heart was larger than usual, and considerably distended; the right cavities were sound; the aorta was now slit open; the portion of it near the arch was thickly studded with irregularly shaped small patches of a white colour: these patches were under the lining membrane, and scarcely elevated it. But the most remarkable appearance was in the portion of the aorta forming its mouth, and in the valves. For a space of from one-half to one inch above the valves, the interior of the aorta was of a vivid deep velvet red, contrasting very strongly with the ordinary pale colour of the vessel higher up; and between the deeply coloured portion and the healthy part the line of demarcation was quite abrupt. The lining membrane, covering the highly vascular part, felt pulpy to the finger, and protruded beyond the healthy surface by the height of one-eighth of an inch; this protrusion was caused by the effusion underneath it of a gelatinous effusion, lying between it and the middle coat of the vessel.* The immediate cause of death was also revealed. One of the semi-lunar valves was ruptured, and evidently recently, for the rupture was jagged and irregular, and sharp at the edges. This valve was singularly formed. It was like a little pouch, or the finger of a glove, projecting down into the ventricle; the bottom of this pouch had given

* The appearances in this case corresponded pretty nearly with the description of the pathology of acute arteritis, given by Mr. Porter, in his paper on Internal Aneurism, in the *Dublin Journal* for November, 1833. His description is this:—"The lining membrane of the aorta was of a bright crimson or carmine colour, raised with small spangles, like patches of a paler and more opaque tint. This vascularity resided principally in the lining membrane, for on stripping off a portion of it, the fibrous tissue, although evidently inflamed, was much paler. The patches above mentioned were caused by deposition of a soft, white, cheesy substance, which were either in the lining membrane, or between it and the fibrous coat; it came off attached to the lining membrane."

way, and the heart was distended by the blood which had retrograded into it from the aorta. The texture of all the valves was thin, but healthy.

These two cases of Glynn and T. are, I think, particularly instructive, the patients having died of other diseases, while the change going on in the aorta was in what may be called its acute stage. Glynn died of disease of the brain, while Mr. T. evidently died in the syncope produced by the sudden rupture of the aortic valve.

These four cases constitute what I have called the first class of cases which shew us the disease in its recent state, and before any alteration of structure has as yet taken place, when as yet the change consists only in sub-membranous effusion, or in deposition of lymph, either of which may be removed. The following cases shew us the disease when it has gone so far as to produce irremediable alteration of structure, either in the aorta itself or its valves.

In October, 1832, I saw Mr. Flanagan, ætat. 45, a respectable manufacturer. He was then dying with the usual symptoms and physical signs of permanent patency of the aorta, accompanied with effusion to great amount into both pleuræ. The symptoms it is not necessary to detail. The commencement or the onset of his illness is the most interesting to us at present. About three years before this period he was attacked with what was supposed to be nervous angina. He was frequently seized, particularly when walking, with fits of dyspnœa and palpitation, accompanied with pain or oppression, referred to the region of the heart. These attacks obliged him to stop and take rest, until the fit passed off. The attacks became gradually more frequent, and at length the organic disease under which he sunk shewed itself unequivocally.

On dissection, the semi-lunar valves of the aorta were found perforated and cartilaginous, and the lining membrane of the aorta, from the mouth of the vessel to its arch, contained underneath it innumerable atheromatous depositions.

In this case it is hard to resist the conclusion, that had the fits, erroneously supposed to be nervous angina, been referred to their true cause, the inflammatory action going on in the mouth of the aorta might have been arrested and life have been saved.*

Of the third class of cases, or those illustrating the treatment, I have as yet but two instances, but those are satisfactory.

In February, 1835, Mr. D. passed through a very severe attack of acute rheumatism, and in the course of it was frequently seized by what were supposed to be fits of spasmodic dyspnœa. I saw him in his convalescence. He was suffering little from articular pains, but there was very strong action of the heart, with indistinct bruit de soufflet, and exercise brought on severe palpitations. I warned him of his danger, but in vain. He was so impressed with the conviction of his heart symptoms being nervous, that no persuasion could induce him to submit to treatment conducted on any other supposition. In eighteen months afterwards he came to town to consult me ; his lips were livid and his feet œdematous, and he was suffering from severe and frequent paroxysms of dyspnœa. He dreaded to lie down, and the fits of palpitation and dyspnœa were brought on by any exercise, but more particularly by attempting to walk up an ascent. The abdominal organs were sound. The chest sounded well on percussion ; and the respiration was, in every part, natural or somewhat peurile. The pulsation of the heart was felt over a large space with strong impulse, and there was very slight bruit de soufflet in the præcordial region ; pulse was about eighty-five and small. These were the symptoms on the 4th of August,

* Through the kindness of Dr. Marsh, I saw a case very similar, in which the symptoms were also those of angina pectoris, accompanied with acute pains shooting to the shoulders. The patient, in addition to presenting the usual signs of permanent patency of the aortic opening, suffered dreadfully from fits of dyspnœa, agitation, and excessive distress. On dissection the aorta was found studded all over in its arch and ascending portion with white cheesy or fibrinous spangles. It was dilated and its texture was softened.

1836. Leeches were applied over the region of the heart, and ten grains of Hyd. c. Magnesia, given three times a day; while abstinence from wine and from stimulant antispasmodics was strictly enjoined. A seton was inserted below the left mamma.

In four days the mouth was made sore, and the change in his state was as gratifying as it was rapid. The breathing became easy, the fits of dyspnœa ceased, and he slept without disturbance and without dread in the recumbent posture.

On the 22nd I again made his mouth sore. He then returned home, and has never had any return of his former distressing symptoms. While these sheets are going through the press, October, 1837, I have again had an opportunity of seeing him. He is in perfectly good health, after having undergone the exciting and arduous exertions attendant on taking an active part in two of the most violently contested elections in the kingdom.

— Collins, ætat. thirty-eight, was admitted into Jervis-street Hospital on the 8th of July, 1837. His feet, face, and hands were slightly œdematous, and his countenance was peculiarly anxious in its expression. He complained of frequent coughing, but what distressed him most were attacks of palpitation and oppression in his breathing. These paroxysms were frequent in the night, and for the fortnight before admission he scarcely ever lay down, his dread of their coming on was so great. His pulse was regular. The respiration was natural through the chest, and no trace of disease could be detected in the kidneys or digestive organs. It was only on accurate examination of the heart, that slight bruit de soufflet was discovered. It became more audible on making him take a few turns round the ward. He was leeches and blistered over the region of the heart, and put upon blue pill. His mouth became sore at the end of three days; and, in a week from his admission into hospital, the œdema had disappeared, the fits of dyspnœa and orthopnœa had ceased, and he was free from every inconvenience except that produced by the sore-mouth.

I have not dwelt on the cautions requisite to be observed in

making the diagnosis of this disease. These cautions will occur to every experienced and practical stethoscopist. It is obvious that where, as in the cases related, there is but one *positive* symptom, viz., the recurrence of fits of angina, the diagnosis must depend very much on negative evidence, and that to arrive at the diagnosis of the disease, we must most carefully ascertain that there is no other disease in thorax, abdomen, or head, to which the fits of dyspnœa can be referred. In this disease, as in pericarditis, we must often arrive at its diagnosis rather by ascertaining that other diseases, which might give rise to similar symptoms are absent, than by the sufficiency of direct signs produced by the disease itself.

There are some circumstances connected with the diagnosis of the disease, which it may, perhaps, be well to notice, as they might be calculated to mislead. It would not be expected *a priori*, that without sufficient mechanical obstruction, inflammation of the lining membrane of the aorta should produce fits of dyspnœa. The same circumstance, however, is observed in pericarditis. It is not unusual to see patients in the progress of acute rheumatism, suddenly seized with pericarditis, and presenting dyspnœa as the only positive symptom of the attack, and until recent investigation gave us the valuable aid of physical signs, in revealing the true cause of those fits of dyspnœa, they were too often treated as nervous paroxysms, and sudden death, or a lingering heart disease, was the consequence of the mistake.

The observations of Andral, on a case of pericarditis, confirm the observations made here on the connexion between inflammatory affections of the heart, and dyspnœa, as a symptom of them.

“ Dans le cas qui vient d’être cité, la dyspnée est le seul signe qui reste, comme pouvant annoncer qu’il y a affection des organes thorachiques : en procedant *par voie d’exclusion*, comme nous l’avous déjà dit, ou peut encore, dans ce cas, arriver a reconnaitre, ou du moins a soupçonner l’existence d’une pericardite.” The occurrence then of fits of dyspnœa, as a symptom of inflammation of the internal serous membrane of the aorta or

heart, is in analogy with what we observe of it, as a symptom in inflammation of the external serous membrane.

The recurrence of the symptoms in paroxysms is another circumstance calculated to lead an observer away from the true nature of the disease, inducing him to suppose, that where the symptoms are transitory, the diseased action producing them could not be permanent; but this connexion of persisting disease with intermittent symptoms, is only one of a vast number of similar examples that might be adduced. The paroxysms of dyspnœa, in confirmed valvular diseases, the epileptic paroxysms dependent on tumors in the brain, or chronic meningitis of the spinal cord, the spasmodic colics accompanying mesenteric disease, are all similar examples of persisting diseased action giving rise to intermittent symptoms.* That the usual constitutional febrile symptoms of local inflammatory action were not present in the case related, will not surprise any one conversant with the diseases of the thoracic organs, in which it so often occurs, that violent inflammation or effusion may occur without what are ordinarily called the constitutional symptoms of local inflammatory action. The non-accompaniment of constitutional symptoms does not alter the nature of the diseased action, neither must it forbid the requisite treatment.†

In summing up the cases which I have related, I think I may be justified in drawing these conclusions:

* The occurrence of symptoms such as are described, coming on in paroxysms, seems to have a more intimate connexion with aortitis than has perhaps been hitherto suspected; and this connexion would seem to extend to aortitis generally. Bertin, p. 35, gives the case at length of a woman, ætat. 47, who, for two years, suffered under symptoms, supposed by all who saw her to be nervous spasms, and for which she was unsuccessfully treated by antispasmodics. On post mortem examination, the whole tract of the aorta was found inflamed with deposition of lymph, &c.

† Mais la marche différente d'une maladie n'en change pas la nature: soit donc que les diverses alterations de la membrane interne du cœur soient précédées dans leur formation par des symptômes manifestes d'inflammation soit que ceux ci restent latens, leur cause leur origine première ne nous semblent pas devoir être regardées comme différentes.—ANDRAT, *Clinique Médicale*, vol. i. *Maladies de Poitrine*, p. 54.

1st. That in some cases of what are called Angina Pectoris the paroxysms of dyspnœa, anxiety, mental distress, &c. constituting a fit of Angina Pectoris, and often supposed to be merely nervous, are really the symptoms of aortitis, or inflammation of the mouth of the aorta.

2nd. That the treatment in such cases is the adoption of local bleeding, counter-irritation, and the exhibition of mercury, which experience has taught us are the means best calculated to prevent the effusion or cause the absorption of lymph.

The pathology of the disease, which these cases have enabled us to trace, encourages us also to put into requisition our treatment, and to persevere in it after even a considerable lapse of time has passed by, in instances too where, without this knowledge, we should have looked upon the case in despair, from the belief that irremediable organic disease had been established. We learn from those cases, that the disease may remain for a very considerable time in its first stage, or that of effusion of lymph, without producing any organic alteration in structure. Thus, in the case of Glynn, three months had elapsed between the commencement of his illness and his death, and yet the diseased action had proceeded no farther than the formation of gelatinous effusion under the lining membrane of the aorta. In the case of Mr. T. six months had passed by, and yet the disease was in the same stage; and in the case of Mr. F. nearly three years had elapsed before the disease assumed its confirmed organic character, while, in the case of Mr. D. J. the appropriate treatment was perfectly successful after the expiration of eighteen months.

The detection of diseased action in the lining membrane of the heart or aorta in the early stage, while it is yet under the control of treatment, is a matter of great practical importance. Mr. Wardrop observes, that "it remains an important desideratum to discover the means of discriminating diseases of the heart in their earliest stages, and before any of these formidable changes of structure have taken place, which it is not within the powers of medicine to remove. And the detection of these (the

symptoms) will enable us to apply remedial means, with a probability of achieving the same advantages as in the treatment of diseases of the organs." I hope the cases related here may contribute some little to enable us to make the diagnosis of disease in so important an organ at that early stage when the diagnosis can be practicably useful.

The accompanying plate represents the disease. A is the portion diseased round the mouth of the aorta: the lining membrane being crimson red, turgid, and swoln out by an effusion of lymph behind it. B represents a portion of the lining membrane of the ventricle, thickened, opaque, and milky, having evidently undergone a similar diseased action, but of longer duration.

This latter affection or endocarditis, and its influence on the muscular action and tissue of the heart, will, I hope, when I shall have accumulated sufficient materials, form the subject of another communication.

ART. XV.—*Observations on the Nature of Neuralgia, and on the Principles, according to which the Treatment of it ought to be conducted.* By JONATHAN OSBORNE, M. D., Fellow of the King and Queen's College of Physicians; Physician to Sir Patrick Dun's and Mercer's Hospitals, &c.

NEURALGIA is a painful affection of a nerve, existing independently of any morbid condition of the surrounding parts. That all pain is received through the medium of the nerves, is proved by the fact, that where they do not exist, there is no sensation; by sensation in the healthy state, being very much in proportion to the supply of nerves sent to any given part, and also by the pain attending inflammation of certain parts, being generally in the same proportion while other circumstances are similar. The nerves, then, being the organs of sensation, and consequently of pain, every painful affection may be said to be neuralgic.

The term is now, however, by common consent, restricted to those cases in which there is no visible disease in the seat of the pain, and in which, for want of any better explanation, a primary affection of the nerves, as the organs of sensation, is supposed to exist.

It may very justly be reproached to physiologists, that when perplexed to account for any of the phenomena of the living body, they refer it to the nerves, in the same manner as chemists, when in a similar predicament, fall back on electricity or galvanism. In the present instance the painful affection has been generally described, not only as an affection of the nerves, but as arising from irritation. Its history may be reviewed very briefly. It appears to have been first noticed by Andre, a surgeon of Versailles, in 1756, who related some cases of *tic douloureux* at the end of his treatise on the urethra, but they, from the unsuitable manner in which they were introduced, appear to have been quite neglected. In Dr. White's work on *Nervous Diseases*, published in 1765, painful affections in the direction of the nerves are very plainly pointed out, but are ascribed to irregular forms of gout or rheumatism. From Dr. Fothergill's paper, published in 1775, (vol. v. *Med. Obs. and Enq.*) we are to date the distinct recognition of *tic douloureux* as an affection of the nerve. Chaussier and Halliday, in France, Meglin in Germany, and several observers in England traced the same affections in other nerves besides those of the face, and in the last edition of Dr. Mason Good's *System*, neuralgia of the face, the foot, and the breast, is distinctly recognized. The cause of the disease was suspected by Fothergill to lie in a cancerous diathesis. Dr. Trevor, of America, held it to be an inflammation of the periosteum of the part affected, while Cotugni, having found an infiltration under the neurilema of the sciatic nerve in one case, and others having said that the nerves suffering from it had been found red and swollen, it was for a time thought to be caused by inflammation of the neurilema.

After a due consideration of the recorded facts, and a comparison of them with the cases under my own observation, I have

for some time past arrived at the conclusion, that the nature of neuralgia is altogether different from any of the opinions respecting it now mentioned ; and that it is nothing else than *pain arising from paralysis of the nerve sufficient to alter its mode of sensation, but not so complete as to obliterate it.* The facts which appear to me to support this view of the subject are the following :

1st. An individual may at any time produce neuralgia in a nerve which is so situate as to be pressed on : for example, let one leg lie over the other so as to compress the popliteal nerves. In due time total paralysis, both of sensation and motion, will be produced, and during the process of becoming paralysed, as well as during that of being recovered from that state, and at the same stage of diminution of sensibility, a peculiar tingling sensation will be perceived along the nerve, often most acutely painful, which all the sufferers from neuralgic pains, with whom I have conversed, at once acknowledged to be, although transient, yet identical with what they suffer.

2nd. The accompanying affections are of a paralytic character ; witness its connexion with partial paralysis of the nerves of the face, which often occurs, as in the case which I am about to relate, and the same in the limbs, of which I have an instance at present in Sir Patrick Dun's Hospital ; witness the neuralgic pains, chiefly in the lower extremities, which are observed in low fevers, when the patient is emerging from a state of coma ; witness also the pains of the same kind (sometimes fatally mistaken for flying gout) which occur in old persons, and which are the result of a slow process of disorganization of the brain, sooner or later to be manifested more clearly, by gradual loss of memory, and at length by those well marked paralytic symptoms, which belong to softening of the nervous structure.

3rd. The *adjuvantia* and *lædientia*. For the alleviation of pain, we are instinctively directed to the use of sedatives, and in neuralgic affections, there is no doubt that they have had an ample trial. But though their failure is so well known, that they are not now recommended with any confidence, yet

this exception to the ordinary laws of the action of remedies does not appear to have engaged attention, or even to have excited suspicion as to the true nature of the affection. All the well established remedies for the cure of neuralgia are of the tonic or stimulant class ; such are arsenic, quinine, iron, &c. If neuralgia be a state of excitement, it is contrary to analogy that it should be treated most successfully, as it is, by those remedies which are known to be most injurious in all other states of excitement. The obvious conclusion is, that it is not a state of excitement.

4th. Inflammation of the nerve, or rather of its sheath, is already well known, and marked by the appropriate symptoms, as pain constant, increased by pressure, accompanied by more or less heat and redness, together with a hardness, as of a cord, in the direction of the nerve, and is therefore an affection distinct from that before us, and with which the latter ought never to be confounded.

5th. The cases of neuralgia on record, in which irritation of the nerve was supposed to have been proved, in consequence of the appearances on dissection, are clearly to be explained by a state of pressure on the nerve, a pressure insufficient to produce total paralysis, and resembling that produced by one leg overlying another. I allude in particular to the case described by Sir Henry Hallford. In this, a morbid growth of bone pressed on the trunk of the infra-orbitary nerve, when in the foramen. Nor is the case described Mr. Jeffries less to be ascribed to pressure. " A young woman, when only six years old, fell down with a teacup in her hand, which was broken, one of the cheeks lacerated, and a fragment of the teacup imbedded under the skin. The wound healed, though slowly, and with difficulty ; the buried fragment of the teacup was not noticed, and was consequently not extracted. From an early period, a violent nervous pain returned nightly, and one side of the face was paralytic. These dreadful symptoms were endured for fourteen years, at the end of which time, an incision was made through the cicatrix, down

upon what was then found to be the edge of a hard substance, and which appeared, when extracted, to be the piece of teacup above noticed. From this time, the neuralgia and paralysis ceased, the affected cheek recovered its proper plumpness, and the muscles their due power." Here the fragment of the teacup, if acting as an irritant, would have produced convulsive motions, instead of which it produced paralysis. Does not this argument, founded upon experience in other cases, lead to the conclusion that it acted solely by pressure? I have had under my care, cases of fungus hæmatodes, and other morbid structures, gradually increasing, and pressing on the parts about the orbit, in which the neuralgic pains, shooting into the direction of the nerves, became less violent, and at length ended in numbness, according as the disease progressed; a change (if my views are correct) to be explained by the pressure producing, first, neuralgia, and afterwards, when increased, producing paralysis.

6th. When neuralgia arises from disordered states of the stomach, dependent on indigestion, it is most agreeable to the accompanying symptoms to consider it as a modification of paralysis, for those symptoms are all of that nature. What are the motes in vision but a transient form of amaurosis, in which so many spots in the retina are, for a time, rendered insensible to the rays of light. Deafness is also at times an attendant on this state, while no one complains of morbidly acute vision or hearing, or morbidly increased energy of the moving or sensorial powers, as attendant on indigestion, although such should be expected if the neuralgia belonging to it were in a state of actual excitement.

In such an event as that described by Dr. Wollaston, the same analogy holds; he having at a dinner party disordered his stomach, by eating of a certain dish, when he went to the drawing room was seized with a violent pain in the ankle; he retired, vomited up the contents of his stomach, and was immediately relieved of the pain. In the mere dyspeptic patient the presence of large quantities of the narcotic gases, especially carbonic acid and sulphuretted hydrogen, in the stomach and upper

portion of the bowels, produces both mopes in vision and headache. This state is always relieved by the excitement of taking food seasoned with aromatics into the stomach, thus shewing that those sensations really depend on torpor of the latter organ.

7th. It appears reasonable to consider many pains not hitherto held to be neuralgic, but which occur in local diseases as the results of imperfect paralysis; for example, the well known accompaniment of scirrhus, namely, lancinating pains, coming on periodically. These have all the characteristics belonging to neuralgia, and appear to be produced by the pressure of the gradually consolidated structure around them. A more violent and continuous pain is produced by pressure in the case of abscesses forming under the fascia, and this immediately vanishes as soon as the pressure is removed. Of the same kind are the pains, often most excruciating, attending the growth of aneurisms of the abdominal aorta, especially in the neighbourhood of the coeliac axis, and in places where ordinary pressure is not productive of pain. Pains in the sides, in the direction of the intercostal nerves, which are notoriously refractory to leeches or counter-irritants applied to the seat of pain, have of late years been found to yield to the very same applications made at the origins of those nerves. These affections have been called spinal irritation, without any adequate reason, inasmuch as no irritating cause can be assigned, and are rather to be viewed as the consequence of vascular congestion, pressing on the nerves at or near their origin, and thus causing imperfect paralysis.

8th. When the nerves are divided by a cutting instrument, as in amputation, the pain felt by the patient is described as if the part divided was burned. Here, then, although irritation is immediately applied to the trunk of a nerve, there is no sensation, as of shooting down to the extremities. Afterwards, when the limb is in process of healing, the patient complains of various sensations referred to the amputated limb; thus individuals have been observed to scratch the extremity of a wooden leg, mistaking it for the foot. While those latter instances prove

that sensation is not in the nerve, but at the nervous centre, they at the same time, shew that a feeling can be excited, as if in a part, where, so far from nervous irritation arising, there are no nerves whatever, and this through the intervention of previous associations and recollections.

9th. To avoid misapprehension, it must be observed, that by neuralgia we now understand that painful affection of the nerve, in which the pain proceeds from the trunk to the extremities, being in an opposite direction to that of ordinary sensation, which is from the extremities to the nervous centre. We are not now treating of those cases in which pain along the course of a nerve is produced from injuries at their extremities, and which are undoubtedly the effects of irritation. To shew, however, how closely the two may be complicated, let us consider an ordinary toothache from a carious tooth; here the pain is felt in the pulp of the tooth, and when irritating substances are brought into contact, becomes excruciating, and then the pain shoots upwards, and resembles that which is felt at the moment of extraction. This is clearly pain produced by external irritation. When, on the contrary, the teeth being sound, and without any inflammation, a pain is observed to shoot downwards, not continuously, but at intervals, then the affection is strictly neuralgic, and if my views are correct, arises from a semiparalytic state of the nerve, induced either by the torpifying influence of cold, by indigestion, or by pressure. Both the neuralgic and irritative pains will always be confounded from their often existing together at the same time in the same individual, and the confusion will be increased by the fact, that they are sometimes removed by the one remedy, as for example, in the case now adduced, by the extraction of a tooth, or by external applications.

10th. Many pains, which from their fugacious, and generally transient character, appear to be neuralgic, are truly spasmodic, or to speak more rigorously, they occur in connexion with, and in proportion to a state of spasm. For example, the

pain in wry neck, and the shooting pain of lumbago. In rheumatic fever, the patient is sometimes more harassed with flying pains of this nature, than with any other circumstance attending the disorder. For those I have found an eminently successful remedy in belladonna, which, given internally, in half-grain doses of the extract every third hour, stops those pains, as by a charm, and in a manner the more remarkable, as it has no effect whatever in relieving the fixed pains, or in arresting the course of the disease. In some cases, however, as in that which I am about to describe, the spasm in the part affected by neuralgia was apparently produced merely by the violence of the pain, which itself acts as an irritant, inasmuch as when the latter was to some degree relieved, the former entirely disappeared.

11th. What appears to me, however, the strongest and most decisive argument that neuralgia is a torpid state of a nerve, is derived from the affect of acupuncture, which cannot be conceived to act in any way than as an irritant. Before I introduce to the notice of the reader a case of long standing, rebellious to other means, and removed by this, and which I have selected as a specimen of its powers, I shall very briefly describe the most important facts relating to the history and mode of employment of this remedy.

The insertion of needles into the body appears not to have been practised by the ancients, at least no reference to it is on record. In India, however, it has been in use among the native practitioners, from an old date. Van Rhyne, a Dutch physician, who resided a number of years in the East Indies, first made acupuncture known in Europe, in the year 1676, and Kempfer, in his *Amœnitates exoticæ*, published in 1712, speaks of it as a remedy used in a species of colic by the natives of Japan. The process, however, was disregarded till 1810, when Dr. Berlioz published his observations.* Within a few years, a variety of

* *Memoire sur l'Acupuncture*, Jour. de Leroux xxxviii. *Memoire sur les Maladies Chroniques les Evacuations Sanguines et l'Acupuncture*, 1817.

observations were made on the subject in different parts of Europe, and in 1825, Mr. Churchill introduced the remedy to the British public, in his *Treatise on Acupuncture*. From this period, we find in the medical journals, a great number of cases of cure reported to have been performed by acupuncture, in circumscribed rheumatisms, neuralgia, pertinacious hiccup, headache, toothache, gastrodynia, lumbago, epilepsy proceeding from a fixed point, trismus, painful affection of the testicles, intermittent fevers, gout, hooping-cough, ophthalmia, painter's colic, anasarca, hydrocele, amaurosis, worms, and tympanites. In a short time it has followed the course and shared the fate of many valuable remedies ; it became a fashion, has been recommended in the most varied forms of disease, has in the majority been found not to answer, and is now very nearly laid aside.

The needles used by the Chinese are of gold or silver, and are inserted by means of a small hammer. In Europe, steel needles have been preferred.* I find well tempered sewing needles, of small diameter, with or without small balls of sealing-wax attached to the top, to answer very well, and such are much better than those with metal tops, as the weight of the latter causes them to shake after being inserted, and thus produces a disagreeable sensation to the patient. By some, they are inserted by a rotatory motion with the fore-finger and thumb. I think it, however, preferable to insert them at once, it being not only less annoying to the patient, but also more efficacious. The pain is very trifling, being nearly confined to the skin, and if the patient's attention is otherwise directed, and pressure made by the hand above the point of insertion, it scarcely exceeds the sensation produced by the prick of a pin. In fact, this process of acupuncture is often performed in sport, by schoolboys on each other. With respect to the depth of the insertion, it has been ascertained by the observations of several

* For some ingenious observations on the employment of electricity by needles, by Mr. Hamilton, see the pages of this Journal.

French experimenters, that a needle may perforate every tissue, even the articulations, the nerves, the parietes of the stomach and intestines, and the arteries, without any injurious result. Passing over Carrero, who has detailed cases of animals asphyxied, and restored to life by acupuncture of the heart, we find Cloquet passing needles through the lungs, the liver, the testicles and the intestines, with perfect impunity. I have little doubt that I have repeatedly transfixed some of the larger arteries, and yet no unpleasant consequence has ever resulted. There is, however, one observation, which with me has been without any exception, it is that in an inflamed part, or a part occupied by rheumatism, properly so called, the operation increases the pain, and aggravates the symptoms, and my conviction is, that if the remedy had been confined to genuine cases of neuralgia, in which *alone* I have found it beneficial,* it would have maintained and increased its reputation, and would, at the present time, rank as one of the most valuable additions made of late years to the European practice of medicine.

In applying acupuncture the object should be to transfix the nerve, and in my first set of cases many repetitions were required, in consequence of this not having been effected. In sciatica, in which the patient, when asked to point out with his finger the direction of the pain, gives a correct anatomical demonstration of the course of the nerve from the outside of the hip to the ankle, it is expedient to follow his finger with a pen, marking it distinctly, and then to insert the needles in the line marked. Their number may vary from ten to thirty, according to the apparent exigency of the case, and they may be allowed to remain for about three or four hours. For extracting them with the least inconvenience, a piece of ivory with a slit is useful, as the oxydation of the needles which takes place causes adhesion and pain when they are withdrawn. ♣Acupuncture being, according to

* The above is contrary to the statements of others, equally entitled to credit with myself. "*Dicam quod sentio.*"—CICERO *de Orat.* As a mechanical means of letting out fluid in distention of dropsical swellings, it is often of great value.

my opinion, indicated exclusively in neuralgia, and being in it a most powerful remedy, which rouses the nerve from incomplete to complete sensation; its efficacy is greatest when the nerves themselves are transfixed in the greatest number. In tic douloureux of the face, as the nerves of that part are spread over a greater extent of surface than in the fleshy parts of the lower extremities, which are the seat of sciatica, it is evident that the chance of nerves being transfixed is much less; and, hence, I think acupuncture will in these cases require to be more frequently repeated. The needles which I use in facial neuralgia are less than one-half inch in length, and are generally the pointed ends of well-tempered needles broken off. They must also be inserted in greater number. Not being aware of any previous application of accupuncture in tic douloureux of the face, I now proceed to detail my specimen case, which is remarkable, first, for the connexion of the disease with paralysis; second, for the pertinacity with which it resisted all remedies employed during a long time; and, thirdly, the happy effects which attended the employment of acupuncture.

Mr. M. aged 36, a grocer, placed himself under my care suffering the most acute pain in the side of the face, extending under and behind the ear, and shooting into the orbit. During the last two years the left side, but during four years previously, the right side was the chief seat of pain. His sleep was only obtained during short intermissions of pain, which, although never entirely absent when awake, was yet variable, and at times increased to the utmost degree of violence. The mouth was slightly drawn upwards to the left side; the motion of the opposite side impeded. The tongue slightly protruded towards the left; the right eye directed inwards from the axis of vision. Within about six months from the commencement of his illness, he gradually lost the sight of the right eye, and about the same time the hearing of the right ear. His sight had been recovered, but in the right ear hearing remained defective, with the sensation of a buzzing noise in it. The complaint appeared

to have been occasioned by untimely cold bathing; a number of remedies, including repeated blisters, had been employed under the direction of a most judicious practitioner, but without benefit. He thought that he once was relieved by the application of ice, but heat although often tried had always failed.

Having in the first instance cleared the bowels with calomel, tartrate of antimony, and senna mixture, I commenced the local treatment by rubbing over the affected part the extract of belladonna, which was kept constantly applied. Having on former occasions seen much relief from this in slight cases of fascial neuralgia, it was desirable to see what it could effect in this case of six years' standing. He experienced no appreciable action beyond the prickling sensation it causes on the skin, and the dilatation of the pupil. Acupuncture was then practised behind the ear, and about the zygomatic arch. It was followed by a remission of pain greater than he had hitherto ever experienced. Next day the pain behind the ear again began to be felt: the needles were applied there in greater number. On the following day the pains were reported as fugacious, and rather resembling an apprehension of its recurrence than pain actually present. On several following days he continued in this state, but his nights continued sleepless, although free from pain. Considering that his want of sleep was now kept up principally by the habit of lying awake, I gave him acetate of morphine, in doses of one-sixth of a grain, in solution, every two hours, commencing in the evening. This proving inadequate, he got twenty drops of the black drop. Sleep was at once restored. His general health, which had suffered much began rapidly to amend. The pulse, previously 116 and hard, fell to 84, and became soft. About four nights afterwards the pain re-commenced behind the ear, and further back than when acupuncture had been applied. Needles were applied to that part, with the effect of completely dislodging the pain. It is unnecessary to detail the remedies which were used in the sub-

sequent treatment of this case, as the neuralgic pains from this time never re-appeared.

He had afterwards a pain in the forehead and vertex, but which was different from the former one, being increased by coughing. For this he got pills of calomel and opium thrice daily, followed by purgatives, an application of leeches to the Schneiderian membrane and a sinapism to the nape of the neck. This pain left him in a short time; the neuralgic pains no longer returned, the result of which was soon visible in a complete restoration of flesh and strength. He retained such a lively sense of gratitude, on account of his recovery, that he wrote a letter to a distinguished individual, who formerly held an exalted situation in this country, informing him of the new mode of cure, urging him to try the same, being influenced by the erroneous, but very common supposition, that all diseases called by the same name are to be cured by the same remedies.

I could cite several other cases, many of them in hospital and some in private practice, all showing that stimulation of the nerve is the mode by which neuralgic pain is, with most certainty, to be subdued. This stimulation of the nerve is most readily effected in sciatica, by a row of needles in the direction of the nerve, to which are ancillary rubefacient liniments in the same direction, purgatives, quinine in large doses, small doses of turpentine, a compound of equal parts of Huxham's tincture, and ammoniated tincture of guaicum, and freshly prepared carbonate of iron, from all of which I have observed benefit. There are, however, some cases of sciatica in which I have not been successful with the above or with any other remedies, and on a strict examination I found the unsuccessful cases to agree in one point, that in them the pain along the leg could be excited by coughing, the parts being held steady. This appears to me to arise from the disease being situated either within the *theca vertebralis*, or at least in the pelvis, and being thus in a measure out of the reach of remedies. Not that I mean to say that such cases are not curable, for I succeeded with some, but all

my unsuccessful cases were of this description ; and guided by this experience, while I give a promise of relief in the one set of cases, I hesitate to do so in the other. It is perhaps unnecessary to add, that there are many cases in which neuralgia is to be considered only as a symptom, and dependant on some other disease, to which the treatment should be exclusively directed. This has appeared to me so obvious that I have not noticed it in the preceding pages.

ART. XVI.—*Case of Hæmaturia.* By MARTIN HAMILTON
LYNCH, M. D., of Loughrea.

— Esquire, aged 37, of a spare form, and temperate habits, has never had gonorrhœa, nor has he shewn any symptoms of calculus. About sixteen years since, he laboured under disease of the vertebræ, which has left angular curvature in the lumbar region. The muscles of the right leg and thigh have been wasted, and the movements of the hip and knee joints on that side limited from the time that his vertebræ were affected. During the sixteen years which intervened between the vertebral disease, and the attack to which this communication refers, Mr. — enjoyed good health.

About the 6th of August 1836, Mr. — perceived that his linen was stained with a puriform urethral discharge ; this discharge was not accompanied with any other vesical, renal, or urethral symptoms ; the urine was passed in a full stream, and without pain. The urethral discharge continued for three weeks, it then disappeared, apparently in consequence of the use of sulphate of quinine, of which the patient had taken two grains thrice daily for a week. Mr. — now continued for four weeks free from any complaint, if I except an occasional pain in the region of the kidney, which he described as “darting or stinging.”

Towards the end of September, hæmaturia shewed itself, the urine was deeply tinged with blood, and after having rested

for some time, deposited in the bottom of the vessel a thin layer of grumous coagulum ; no coagulum was at any time passed from the urethra. Mr. — allowed the complaint to continue for two months without having medical assistance ; during this time he followed his usual occupations, and his diet was unchanged, so that there could not have been any very urgent constitutional symptoms. At this period, and during the few preceding months, he had occasion to apply himself to the desk for several hours daily.

About the 25th of November he was seen by an experienced physician, under whose care he remained for some weeks, during which blood was taken from the arm, and purgatives were administered ; also demulcents, alkalies, uva ursæ, the mineral acids, &c., without any effect being produced upon the disease.

I first saw him on the 5th of January ; I found the urine tinged with blood to the utmost possible degree, but no coagula had ever been passed from the urethra ; the grumous deposit before alluded to had increased in thickness ; there was some debility, and considerable emaciation, his pulse beat eighty, and communicated the hæmorrhagic thrill, it was of tolerable strength ; his appetite was good ; there was no thirst ; there was no irritability of bladder ; pressure made upon the pubic region did not give rise to any uneasiness ; the patient slept eight or nine hours without emptying his bladder. There was not the slightest symptom of calculus ; the urine was passed in a full stream ; and the evacuations were so figured as to induce me to conclude that there was no enlargement of the prostate gland. The urine reddened litmus paper, and exhibited but a slight trace of albumen. To the anxious inquiries of the patient's family as to the event, I stated, that the affection was apparently in one of the kidneys ; that there was no symptom which would enable a physician to say positively whether it was functional or organic ; and that if organic it would terminate fatally. I recommended the patient to have eight ounces of

blood taken by cupping from the loins, and to take spirits of turpentine in doses of thirty drops, four times daily with demulcents. Mr. — hesitated to submit to this treatment until it had been submitted for approval to a distinguished surgeon of Dublin, who having been consulted by letter advised him to commence it, and to take a pint of lime water daily with milk.

The following mixture was ordered and taken regularly for nine days.

℞ Spir. Terebinthinæ Rect. ʒiii.

Tinct. Opii gutt. xxx.

Confect. Amygdalarum ʒii.

Lactis Amygdalarum, ʒvi. M.

Sumat cochlearia duo majora quater quotidie.

The lime-water with milk was also used, and flax seed tea drank copiously.

On the ninth day from the commencement of this treatment, the urine was perfectly free from blood, its colour was quite natural; it did not present any trace of albumen on being tested by boiling, and the bichloride of mercury; it changed litmus paper to red. The pulse became natural, and all the functions were regularly performed. There was a healthy interval of twenty-five days, at the end of which the disease returned, but yielded in eight days to the terebinthinate treatment. The next interval of health was of eighteen days' continuance, at the expiration of it the urine was again bloody, but become natural after the turpentine had been used ten days. The next interval was one of five days, at the end of which the disease again made its appearance, and continued to the time of his going to Dublin for the purpose of having a consultation on his case, (7th of April.)

During this new attack, the only additional symptoms were, that long, narrow, membrane-like shreds, *not of a cylindrical form*, and similar to moistened cobwebs, were observed to float

in the urine, and the almost constant presence of severe pain shooting from the lumbar region to the umbilicus on each side.

Mr. ——— made the journey to Dublin, more than eighty miles distant from his residence, without his disease being perceptibly aggravated.

The consultants, three of the most eminent men in Dublin, were of opinion that the bloody urine was produced by organic disease, most probably of the kidney, and that recovery was very improbable. They carefully examined the state of the prostate, and found that there was no enlargement of that gland. The directions were that a few ounces of blood should be taken by cupping, from the loins, and that the acetate of lead should be given according to the following formula :

℞ Acet. Plumbi gr. iij.

Aceti distillati ℥i.

Tinct. Opii gutt. iii.

Aquæ distill. ℥i.

Fiat haustus 8vis horis sumendus.

This draught was taken regularly for some days, when the occurrence of lead colic, with marked evidence of depressed energy of the nervous system, despondency, and tremors, caused it to be omitted.

Mr. ——— was now treated with laxatives, and the muriated tincture of iron, for a few days, when lead was again given in the following form :

℞ Extr. Opii Aquos gr. iii.

Acet. Plumbi gr. ix.

Cons. Ros. q. sut fiat pil. tres.

Capiat unam mane meridiæ nocteque.

But lead again producing disagreeable symptoms, and having no effect on the disease, was omitted. The patient left town, determined to await the result, without submitting to further treatment.

Mr. ——— reached home on the 20th of April, he now

complained much of thirst, he had no desire for food, his tongue was foul, his rest bad, his bowels constipated, his pulse 120, weak, but thrilling, his urine was more deeply tinged than ever, and the abdominal pains very troublesome. His countenance was expressive of great prostration; the debility and emaciation were extreme. For some days the patient could not be induced to take medicine of any kind; he merely attempted, but in vain, to procure evacuations by the use of "lavements." On the 23rd, 24th, and 25th, he was annoyed by a most distressing hiccup, which disappeared for half an hour, whenever he drank a little spring water, or a teaspoonful of vinegar. On the evening of the 25th he took the following draught:

℞ Spirit. Æther. Nitrosi gutt. xxx.
Camph., gr. iii.
Confect. Amygdalar. ℥ii.
Mist. Camph. ℥j. Ft. haustus.

Also on the same evening, three teaspoonfuls of the following electuary:

℞ Elect. Sennæ ℥j.
Elect. Cass. Fist. ℥ss.
Supert. Potass. ℥iii. Ft. elect.

On the 26th there was no hiccup, the pulse had fallen to 108, and there had been four stools; in other respects the patient's state had not improved.

April 27th. On this day I prevailed upon Mr. ——— to submit again to a course of treatment, and I selected alum for two reasons, first, because it had not hitherto been tried; secondly, because the patient still suffered from the deleterious effects of lead. I ordered the electuary to be taken at night, whenever the state of the bowels required it, and the following draught to be administered thrice daily:

℞ Sulph. Alum. et Pot. gr. xv.
Acid. Sulph. dilut. gutt. xxx.
Sulphat. Magnes. ℥i.
Inf. Ros. ℥i. Ft. haustus.

This draught was used regularly to the 15th of May, when the quantity of alum was increased to a scruple in each draught. On the 20th the quantity of alum was raised to half a drachm for each dose. On the 22nd it was raised to forty-five grains, and continued to the 28th.

During the use of the alum the appetite improved, the pulse fell gradually from 108 to seventy-eight, the rest became uninterrupted, and the bowels were freed once or twice daily *without the use of the electuary*. After the alum had been given three days, the urine exhibited a singular change of colour, it was of a deep brown like the strongest porter, and shewed not the slightest trace of red, whether viewed in a glass or an earthenware vessel; there was deposited from it a yellow powder; the urine presented these appearances for a few days, when it became the same colour as pale table beer. From this time the urine became gradually paler, (except now and then when the porter-colour returned for a few hours.)

On the 5th of May it became perfectly clear and in every way natural. The patient has been in perfect health since then, except for four or five days in the commencement of June, when the urine again became bloody, but this attack quickly yielded to the alum given in doses of forty-five grains. Mr. ——— is now in better health than he has been for some years.

REMARKS.—This case is one of some interest as exhibiting the effects of two of the most powerful remedies in passive hæmorrhages, turpentine, and alum. It shews that when an hæmorrhagic affection has reached the “passive” stage, the physician should be extremely cautious in administering lead.

It is to be observed, that, before the alum had produced any effect on the hæmorrhagic affection, the symptoms produced by the deleterious action of lead had yielded. This case also furnishes an instance of the administration of alum for several weeks in large doses, without the occurrence of constipation, no purgative being given during that time except the drachm of sulphate of magnesia in each draught. This combination was

used most beneficially in a case of hæmaturia, treated by Mr. Guthrie several years ago, and published in some of the journals of that period.

It is useful, with a view to the treatment of other hæmorrhages, to bear in mind the extent to which alum may be given without danger of constipation.

To prove that the remarkable change of the colour of the urine to that of strong porter, was not accidental, but depended upon the use of alum, I shall relate an experiment performed by me in the presence of Mr. Walsh, apothecary of this town. I washed the coagulum of blood, drawn about half an hour previously from a bronchitic patient of good constitution, in a mortar, with eight ounces of distilled water, and strained it. Having poured the red fluid, thus separated from the fibrin, into a phial, I added a table spoonful of saturated solution of alum. The blood colour changed in one or two minutes to the darkest brown, the change being accompanied with considerable effervescence.

ART. XVII.—*Second Report of the New Lying-in Hospital, Dublin.* By THOMAS EDWARD BEATTY, M.D., M.R.I.A., Consulting Accoucheur, and late Master of the Hospital; Consulting Accoucheur to the City of Dublin Hospital; Lecturer on Midwifery at the Medical School, Park-street; and lately Professor of Medical Jurisprudence to the Royal College of Surgeons in Ireland.

THE period embraced by the following Report of the practice of the new Lying-in Hospital, Dublin, extends from the 12th of July, 1835, (the date of the former Report, published in the Dublin Medical Journal for September, 1835,) to the 31st of August, 1837; during which time *eight hundred and four* patients have been treated, *three* of them labouring under uterine disease, and *eighteen* being cases of abortion under four months

of gestation, leaving *seven hundred and eighty-three* deliveries at or near the full time.

The form of tables adopted in the former Report has been followed in the present, as it appears to be sufficiently comprehensive, without being too minute, and it affords a facility in summing up the total number of cases treated since the opening of the institution.

Upwards of *six thousand* women and children have been prescribed for at the Dispensary attached to the Hospital, and *three hundred and eighty-four* children were vaccinated.

WOMEN DELIVERED.

Naturally,	.	744		
Artificially,	.	9	By	Turning, 1
		<hr/>		Perforator, 1
Total,		783		Forceps, 7 or 1 in 112
				<hr/>
				9

PRESENTATIONS.

DURATION OF LABOUR.

Head . . .	743	Under 6 hours . . .	329
Face . . .	2 or 1 in 381	Above 6 . . . under 12 .	274
Breech . . .	15 or 1 in $52\frac{2}{10}$	12	24 . 130
Feet . . .	9 or 1 in 87	24	36 . 31
Arm . . .	1	36	48 . 13
Funis . . .	4 or 1 in $195\frac{4}{10}$	48	60 . 3
Head and hand	1	60	75 . 2
Twin Cases .	8 or 1 in $97\frac{7}{10}$	75	96 . 1
Total, 783		Total, 783	

CHILDREN BORN.

Males,	414,	of whom alive,	374,	dead,	40
Females,	377,	.	354,	.	23
Total,	791	Total,	728	Total,	63

TWIN CASES.

Both children presented head in 6 cases.

1st head, 2nd feet, 2

Total, 8

Of the 63 children born dead, there were—

Footling cases, 6	Perforator, 1
Breech, 10	Premature, 9
Funis, 3	Putrid, 8
Twins, 4	Natural, 16
Arm and turning 1	Acephalous monster . . . 1
Ruptured uterus 1	
Forceps, 3	Total, 63

FATE OF CHILDREN IN TWIN CASES.

Both alive in 6 cases, 12

Both dead 2 4

Total, 8 Total, 16

WOMEN DIED.

Of puerperal fever, 8

Hæmorrhage after delivery, . . 1

Ruptured uterus 1

Total, 10

The first point to which I wish to direct attention is, the table indicating the duration of labour; in referring to which, the number of women, whose labour exceeded twenty-four hours, appears to be fifty, or in the proportion of nearly one in sixteen.

My reason for bringing this prominently forward, at present,

is, that since the last Report of the new Lying-in Hospital was published, in September, 1835, Professor Hamilton of Edinburgh has favoured the profession with his "Practical Observations on various Subjects relating to Midwifery," in which he devotes a great many pages to the management of the different kinds and stages of labour. This work, excellent as it is, and coming from the pen of an author, whose great experience, high character, and acknowledged talents, entitle him to the respect and confidence of the profession, contains some doctrines respecting the management of labour, from which, with the greatest deference to his opinion, I feel myself called on to express my dissent.

In many points relating to the management of difficult labour, to which I will have occasion to refer, when I come to notice that part of the Report, my experience leads me to coincide entirely with the learned professor; but, I regret that I cannot equally subscribe to the opinions contained in the following passages, relating to the first stage of labour. In speaking of that part of the process of parturition, Dr. Hamilton states, at P. 188, Part 1: "He observed that when the natural powers are alone trusted to, this stage is often greatly protracted, and he of course inferred that injurious effects must be the consequence." What these injurious effects are supposed to be, we learn at p. 122. Firstly, "That the powers of the uterus may be inadequate to expel the infant with safety to its life, or to the future health of the parent." Secondly, "That after the birth of the infant, the uterus may contract irregularly so as to occasion retention of the placenta." Thirdly, "That after the expulsion of the placenta, the contractions of the uterus may be too feeble to prevent fatal hæmorrhagy." And lastly, "That, supposing the patient should escape all those untoward circumstances, febrile or inflammatory affections of a most dangerous nature may ensue from the previous protraction of pain, and the irregular distribution of blood.

Acting on the belief that the first stage of labour was thus

often greatly and dangerously protracted, the Professor states, p. 195: "From the year 1800 the author has advised his pupils to secure the termination of the first stage of labour within twelve or fourteen hours from its actual commencement;" and further, "by the adoption of this rule, the author can confidently assert, that no patient under his charge for the last thirty-five years, has been above twenty-four hours in labour, and excepting in cases of disproportion, none so long."

In contrasting the result of his practice with the recorded evidence of the protraction of labour in London, Paris, and Dublin, Dr. Hamilton does me the honour to quote my last Report of the New Lying-in Hospital, Dublin, from which it appears that one woman in twenty-one had her labour protracted beyond twenty-four hours. In the present report the proportion is still greater, being, as I have mentioned, nearly one in sixteen, and it has appeared to me, that the best defence of such practice is to give a separate table containing all the cases in both reports, in which the labour exceeded twenty-four hours. The number contained in the former Report was nineteen, which, added to fifty in the present, makes sixty-nine; this is in the proportion of a fraction more than one in seventeen to the whole number 1182.

In the following table separate columns are allotted to the number of hours each woman was in labour, the sex of the child, the fate of the child, and the fate of the mother. A second table is added, shewing the number of women whose labour was between twenty-four and thirty-six hours, and so on; and, likewise, the number of children born alive and dead, and their sex. A short notice of the still-born children follows, shewing the cause of the death of the child in each case.

No.	Hours in Labour.	Sex of Child.	Fate of Child.	Fate of Mother.	No.	Hours in Labour.	Sex of Child.	Fate of Child.	Fate of Mother.	No.	Hours in Labour.	Sex of Child.	Fate of Child.	Fate of Mother.
1	75	F.	A.	A.	24	30	F.	A.	A.	47	72	F.	D.	A.
2	30	M.	A.	A.	25	36	F.	A.	A.	48	48	F.	A.	A.
3	48	M.	A.	A.	26	96	M.	D.	A.	49	36	M.	A.	A.
4	26	M.	A.	A.	27	36	M.	D.	A.	50	48	F.	A.	A.
5	60	M.	D.	A.	28	30	F.	A.	A.	51	48	M.	A.	A.
6	54	F.	A.	A.	29	31	F.	A.	A.	52	38	M.	A.	A.
7	28	M.	D.	A.	30	29	F.	D.	A.	53	40	F.	A.	A.
8	36	M.	A.	A.	31	33	M.	D.	A.	54	26	M.	A.	A.
9	35	M.	A.	A.	32	48	F.	A.	A.	55	60	M.	D.	A.
10	26	F.	A.	A.	33	26	M.	A.	A.	56	36	F.	D.	A.
11	45	F.	D.	A.	34	32	F.	A.	A.	57	36	F.	A.	A.
12	43	M.	A.	A.	35	40	F.	A.	A.	58	25	F.	A.	A.
13	26	M.	D.	A.	36	48	F.	A.	A.	59	136	M.	D.	A.
14	26	F.	A.	A.	37	26	M.	A.	A.	60	96	M.	D.	A.
15	50	M.	D.	A.	38	25	M.	D.	D.	61	25	F.	A.	A.
16	34	F.	A.	A.	39	28	F.	A.	A.	62	60	M.	A.	A.
17	30	M.	A.	A.	40	33	M.	A.	A.	63	25	F.	A.	A.
18	36	F.	A.	A.	41	37	F.	A.	A.	64	48	F.	D.	A.
19	32	M.	A.	A.	42	32	F.	D.	A.	65	36	M.	A.	A.
20	30	M.	A.	A.	43	30	M.	A.	A.	66	28	M.	A.	A.
21	48	F.	A.	A.	44	38	M.	A.	A.	67	48	M.	D.	A.
22	36	M.	A.	A.	45	42	M.	D.	A.	68	30	F.	A.	A.
23	28	M.	A.	A.	46	31	M.	A.	A.	69	28	F.	D.	A.

Hours in Labour.			No. of Cases.	Child alive in.	Child dead in.
Between	24 and	36	41	33	8
	36 and	48	17	13	4
	48 and	60	5	3	2
	60 and	72	1	0	1
	72 and	84	2	1	1
	84 and	96	2	0	2
	96 and	136	1	0	1
			69	50	19

Of the nineteen children born dead thirteen were males.

Of the eight children born dead under thirty-six hours of labour, one was in a case of convulsions occurring in a first labour, at the end of twenty-five hours, when delivery was effected by the forceps; two were cases of presentation of the breech, attended with much difficulty in passing through the pelvis; one was a case of placenta presentation, one was in a case of ruptured uterus, and three were in cases of difficult labour, in two of which the delivery was assisted by the forceps.

Of the four born dead under forty-eight hours, three were in cases of uncomplicated difficult labour, one was delivered by the forceps.

Of the two born dead under sixty hours, one was a breech case, the other a natural presentation.

The one case born dead under seventy-two hours was a breech presentation.

The one under eighty-four hours was in a case requiring delivery by the perforator.

Of the two born under ninety-six hours, in one case the mother was four days in labour before admission—delivery accomplished by the perforator. In the other the labour, though constant, was not severe.

The one case in which the labour lasted 136 hours was terminated by the perforator. The details of this case were given in the last Report of the Hospital.

From this table, faithfully extracted from the Hospital book, it appears, that of the sixty-nine women whose labour exceeded twenty-four hours, only one died, No. 38, and her's was a case of ruptured uterus; that fifty children were born alive, and nineteen dead, thirteen of whom were males; and that of the nineteen children still born, *one* was in a case of convulsions, *one* in a placenta presentation, *one* in a case of ruptured uterus, *four* were breech presentations, *three* were delivered by the perforator, and the remaining *nine* were ordinary head presentations.

From this record I think I am justified in saying, that pro-

traction of labour beyond twenty-four hours is not *per se* productive of those injurious effects described by Dr. Hamilton, and that many women may be allowed to go beyond that time with safety to themselves and their offspring. I believe there are few practitioners in this country whose experience could not furnish many instances of labour happily concluded for both mother and child at the end of thirty-six or forty-eight hours, and I make bold to say, that in no country is the mortality attending lying-in women less than in Ireland.

The opinion therefore advanced by Professor Hamilton, that the first stage of labour is often greatly protracted when the natural powers are alone trusted to, and that when it is, injurious effects must of course be the consequence, seems to me objectionable, because it leads to interference in many cases in which it is manifestly unnecessary, and it is calculated to introduce what has been so well decried by Dr. Blundell, "a meddlesome midwifery." The effect of such a recommendation on the mind of a young and inexperienced practitioner must be to induce him to harass his patient by frequent examinations during the early stage of her labour, a practice which is always prejudicial to the patient, by producing excitement and irritation in the vagina and os uteri, and thus causing what we would desire to avoid, viz., a rigid condition of the parts. It is likewise calculated to excite an undue degree of restlessness and anxiety on the part of the attendant, and to induce him to have recourse to blood-letting, &c. in many cases where such proceeding is uncalled for, and by the alarm thus produced in his patient's mind, to suspend or materially interrupt the proper course of labour. Let it not be imagined that by the foregoing remarks I wish to dispense with, or undervalue blood-letting, opium, &c. in cases demanding their employment. What I desire to combat is, the doctrine, that *all* labours shall be reduced to the same limit, and that the period of delivery is to be measured out by the clock, allowing a certain number of hours to the performance of certain stages of the labour in *all*

cases, I object to this because I know that the powers of endurance are as various as the constitutions of patients, and that one woman will bear with impunity a labour under which another will sink. Symptoms then, and not time, are the guides to which we should look for the regulation of our practice, and upon that doctrine the treatment of patients in the New Lying-in Hospital has been founded ; with what success, is now laid before the profession.

Before quitting this subject I think it right to mention that probably there is not so great a difference between the length of time that Dr. Hamilton's patients and those in this city are allowed to continue in labour, as he supposes. In this Report the labour is dated from the first symptoms, and all suspensions are included ; while Dr. Hamilton says, p. 223, Part I, " It is certainly possible that after the first stage is fairly begun it may be suspended for some hours, the uterine contractions no longer recurring. If during this interval, there be no injurious pressure upon any part of the mother, the previous pains are not to be reckoned, but the duration of the first stage is to be dated from the recurrence of the pains." By following this rule, many hours of real labour may be subtracted, and thus a really long labour may be made to appear a very short one. For example, I attended a lady some years back, who, although recorded in my registry as having been twenty-fours in labour, would be rated, according to Dr. Hamilton's rule, at half an hour, because having been seventeen hours in labour, the pains subsided, and she had none for six hours, when a dose of ergot of rye restored uterine action, and she was delivered in half an hour afterwards. In such a case as this I think it would be unfair to omit all the previous suffering in calculating the length of labour, and I allude to it for the purpose of shewing, that the difference in the time that Dr. Hamilton's patients and women in this city are allowed to remain in labour, may not be so great as he supposes, but may be more apparent than real.

INSTRUMENTAL DELIVERIES.

I now pass to the consideration of those cases in which it appeared necessary to assist the natural efforts of the uterus by artificial means. These are eight in number, seven in which the forceps were employed, and one where it was judged imperative to use the destructive instruments.

Before I detail these cases I think it right to state, that though I have felt it necessary to disagree with the opinions advanced by Professor Hamilton on the subject of ordinary labour, it gives me much pleasure to express my entire concurrence in his views and doctrines respecting the management of laborious labours, and it will appear on perusing the history of the following cases that the practice pursued was just that so ably and clearly recommended by Dr. Hamilton. Nothing can be more judicious than the following: "On the whole it may be concluded, that so long as there are no untoward symptoms in respect to the general health—so long as the pains continue to advance the infant—and so long as the passages remain in their healthy natural state, the contractions of the uterus may be expected to complete delivery. But whenever symptoms of derangement of the general health, or evidences of the uterine contraction ceasing to advance the infant, or of there being an impediment to its advance in consequence of some state of the passages, become apparent, and more especially, whenever circumstances denoting injurious pressure, or interrupted circulation in the important parts concerned in parturition, occur, the natural efforts can no longer be trusted to."—Page 51, Part 2. And again, "The obvious duty of the practitioner in every case must be to ascertain what the natural efforts can accomplish, and when he is satisfied that the delivery can not be permitted to go on without some injury to the mother, or to the infant, he is no longer to delay assistance; keeping this principle in view, the time allotted for the efforts of nature must be regulated by the symptoms of the individual

case.”—Page 97, Part 2. With respect to the kind of aid to be afforded we have the following rational and important observations: “He considers that after the second stage has commenced, if regular pains continue and the infant become wedged in the passage, the practitioner is imperiously called upon, supposing the infant to be within reach of the forceps, to interfere, before there is a probability that the pressure may destroy the infant’s life, and certainly before any untoward symptom threaten the mother. The forceps, if properly applied, can do no harm whatever to the mother, while, by diminishing the bulk of the infant, it enables the practitioner to lessen as well as shorten her sufferings.”—Page 106, Part 2.

I have said that the practice at the New Lying-in Hospital was founded on such principles as these, and I now proceed to relate the cases as they occurred, and the result of them.

CASE I.—Bridget Boland, aged twenty-five years, first pregnancy, delivered October 10th, 1835. The head presented in the first position, and the labour progressed steadily but slowly, so that at the end of twenty-four hours, the os uteri was completely dilated, and a portion of the head had passed the brim of the pelvis. The pains continued severe and constant, and at the end of thirty hours, the greater portion of the bulk of the head had passed into the cavity of the pelvis, but not so low as to occupy the hollow of the sacrum, or make any pressure on the perinæum. From this time there was no advance in the progress of the head, but a tumour began to form on the scalp, which became very large before delivery, and from its bulk was very likely to mislead a superficial inquirer as to the advance of the presenting part. In this condition the head remained for six hours, notwithstanding frequent and strong pains. Seeing that the natural efforts were apparently unavailing to accomplish delivery, the pulse of the woman becoming frequent, and a degree of restlessness (which is so often the precursor of bad symptoms) having come on, and finding by the stethoscope, that the child (notwithstanding the pressure it had undergone for thirty-

six hours) was still alive, I determined to give it a chance for life, and deliver it by the forceps. The instrument was applied slowly and cautiously, without using any force that could injure either mother or child ; and when the blades were locked, the junction lay within the vagina, and the handles pressed upon the *fourchette* ; one blade lay towards the symphysis pubis, the other towards the sacrum.* I am thus particular in describing the position of the instrument when applied, as it will convey a good idea of the part of the pelvis the head was lodged in, and the portion of the head that had passed the brim. So far I felt certain that no injury had been inflicted on either mother or child, and being determined to proceed *non vi sed arte*, it now remained to try whether the amount of force I felt justified in using, would be sufficient to extract the head. The blades lay one to the pubis, the other to the sacrum, consequently, in the direction of these points, lateral motion of the instrument could alone be produced. Grasping the handles loosely, so as not to make too great pressure on the head, I swayed them backwards and forwards two or three times during each pain, without using much extractive force. By this means, having loosened the head in its position, I was enabled to cause it to descend slowly at each return of pain, by gently assisting the uterus ; taking care to apply the force in the direction of the axis of that part to the pelvis in which the head was

* The instrument employed was the short straight forceps, measuring, total length, $10\frac{1}{2}$ inches ; length of blade from upper part of the lock, $6\frac{1}{2}$ inches ; length of handle 4 inches ; widest part between the blades when closed 3 inches ; outside the blades between the same part $3\frac{1}{4}$ inches.

I think the proportions of Dr. Hamilton's forceps better adapted for general use than the instrument above described ; it is longer, and not so wide between the blades. While upon this subject, I wish to mention that many of the forceps to be found in the cutler's shops are dangerous to use in consequence of the sharpness of their edges. Care should be taken that no sharp edge should exist either on the outside or in the fenestrum ; the former may injure the mother, the latter may cut the scalp of the infant in the act of extraction.

placed. Finding a manifest advance, I was encouraged to persevere, without being at any time tempted to make use of such power as could inflict any injury, and after twenty minutes' patient endeavouring, I was gratified by the delivery of a living boy, without a fibre of the perinæum having been damaged. In this case, I would have felt myself bound to desist, if I had experienced any considerable resistance either in the introduction of the instrument, or in the attempt at extraction.

The mother recovered without an unpleasant symptom, and left the hospital, with her child, at the end of ten days.

CASE II.—Mary Brady, aged 24 years, first pregnancy, delivered October 16th 1835. The circumstances of this case resembled very much those of the case just detailed, with this difference, that in the present one, labour was allowed to go on to forty-eight hours before recourse was had to operation, whereas the last was delivered at the end of thirty-six. The reason was that urgent symptoms did not manifest themselves so soon as in the former instance. The pulse kept moderate until within a few hours of the time at which she was delivered, and the os uteri, which was very slow in dilating, had not permitted the head to pass through until nearly twenty-four hours had elapsed. From that time a slow advance of the head took place, but at the end of thirty-six hours not more than one-third of it had passed the brim of the pelvis. In four hours more about one-half had been forced through, and from that time little progress was made, notwithstanding strong and regular pains continued. Still, as no unpleasant constitutional or local symptoms were present, I contented myself in watching the case, so as to be ready to act as soon as required. The patient's strength now began to decline, and her pulse rose to 110, the vagina at the same time began to lose the moist cool feel it had hitherto preserved, and to become hot and dry. When matters had thus begun to change, I felt that I was no longer justified in withholding assistance, and finding the child alive I determined on using the forceps, notwithstanding that

I could not feel the ear, nor was the head near the perinæum. I accordingly proceeded, in the presence of Dr. Brown and Mr. Armstrong, who happened to visit the hospital at that time; and I think it right to mention, that from the high and apparently locked position of the head, both of these gentlemen were doubtful of the success of the operation, neither was I sanguine myself, and I mentioned to the pupils that it was by no means certain that I would be able to effect delivery in that way, nevertheless I felt myself bound to make the attempt under the prudent restrictions as in the last case. By a little management I was enabled to slide the blades of the instrument up to their proper situation, and by degrees assisting the pains when they occurred, I was enabled to deliver the patient safely of a living girl. They both left the hospital well on the twelfth day.

CASE III.—Winfred Meeton, aged 28 years, seventh pregnancy, delivered October 18th, 1835. This woman had borne six children, in the delivery of which she had always difficult labour, and the last child was extracted elsewhere by the perforator and crotchet. In this labour, the head presented in the first position, and great part of it was forced through the brim of the pelvis, which was evidently diminished somewhat in its antero-posterior diameter. A considerable opposition to delivery, also, arose in this case, from a convergence of the spinous processes of the ossa ischia, which encroached upon the cavity of the pelvis, and resisted the passage of the head. Finding at the end of twenty-four hours' hard labour, that there was little prospect of delivery by the natural efforts, and having the former labours as a guide, I determined not to wait too long without attempting to rescue the child from its perilous situation, if it were consistent with the mother's safety. Accordingly, the forceps was cautiously introduced, and as cautiously used as an extractor, and in half an hour from the commencement of the operation, a living boy was born, who, with his mother, continued to do well, until they left the hospital on the tenth day.

CASE IV.—Sarah Collins, aged 36 years, ninth pregnancy, delivered September 30th, 1836. In this case, the head presented in the fourth position, (*Naegelé*), and the labour was very severe from the beginning. At the end of twenty hours, the head was firmly fixed in the pelvis, with the anterior fontanelle towards the pubis, and in six hours more, finding that no advance had taken place, and the vagina was becoming tender to the touch, while the pulse was gradually increasing in frequency, the delivery was effected by the forceps, the child being dead. This patient made a good recovery.

CASE V.—This was the only instance in which I ever knew it necessary to assist the delivery of the head in a breech or footling case, by instrumental aid. Catherine Connor, aged 30 years, delivered November 12th, 1835. The child presented by the feet, and the labour went on without any remarkable circumstance, until the arms had been got down after the expulsion of the breech, when great difficulty was found in the passage of the head. This was not owing to any improper force used in pulling at the body of the child, for I happened to be in the ward at the time of delivery, and superintended the management of the case myself. The labour pains continued strong, and the cord pulsated for a considerable time, during our endeavours to extricate the head. But in all my attempts to accomplish it I failed, and at last, after more than half an hour, finding the circulation in the funis became weak, I determined on using the forceps. Unfortunately the instrument was not in the hospital, and I had to send to my own house (a very short distance) for it; this caused a trifling delay, which, however, was fatal to the infant, for before the messenger returned, pulsation had entirely ceased. I passed the blades along the sides of the head, having the body of the infant carried forward between the thighs of the mother, and with some difficulty I succeeded in extracting the head. We tried all means to resuscitate the infant without success.

CASE VI.—A case of convulsions, to be detailed in another part of the Report.

CASE VII.—Delivery was accomplished in this case in consequence of rupture of the uterus, under which head it will be found.

DELIVERY BY THE PERFORATOR.

CASE I.—Rose Aidy, aged 27 years; first pregnancy, delivered March 19th, 1837. This patient was admitted on the 16th, having had pains for two days previously. She was of low stature, and in external configuration corresponding with the description I gave in the former Report, as one indicating a difficult labour, viz.: “A low, thickset, brawny frame, with broad shoulders and thick limbs, short stumpy fingers as if the last joint of each was cut off, very rigid muscular fibre, and nates so large as to render examination per vaginam difficult.” On admission the pains were slight, the os uteri dilated to the size of a sixpence, with sharp and thin edge. The abdomen very prominent and conical, pulse 80.

17th. Had some sleep, the pains regular, but not severe; pulse 80; os uteri dilated to the size of a shilling, still very rigid and sharp.

V. S. ad. 3xvj.

R. Ant. Tart. gr. ij.

Aq. font. 3viij. sumat cochl. i. amp. omni hora.

18th. No further dilatation of the os uteri. She complains of soreness of the vagina on examination. The bowels not having been freed, two purgative enemata were administered. Ten o'clock P.M., pains stronger, os uteri of the size of a half crown, the edges thicker, but very rigid, no advance of the head. An enema with tinct. opii gutt. xl. was administered, which was speedily rejected.

19th. Had some sleep, pains more frequent and severe. The waters were discharged spontaneously during the night. Os uteri not more dilated, complains of soreness of the belly on pressure; pulse 84.

V. S. ad. 3xxiv.

Haust. c. tinct. opii. gutt. xxx.

The fœtal heart was heard at the right side below the umbilicus. Four o'clock P.M.; has had no pains since twelve o'clock, has slept some, and passed water, os uteri a little more dilated, and the infant's head more forced into the brim of the pelvis; a tumour of the scalp projects through the os uteri; pulse 92. Nine o'clock P.M.; strong and frequent pains since last report, os uteri dilated. The tumour on the head very large, and the right parietal bone overlaps the left considerably, but there is no advance of the bulk of the head through the brim of the pelvis. Blood trickling from the vagina, and the discharge fœtid. She has vomited repeatedly a dark-coloured fluid during the last few hours, and there is great tenderness of the belly; pulse 120. Considering these symptoms, and that she was now eighty-two hours in labour since her admission, I judged it not safe to allow her to pass another night without being delivered, and the head being quite beyond the reach of the forceps, I was obliged to have recourse to the perforator. Great difficulty was experienced in getting the head through the pelvis after its contents had been discharged, and nearly an hour was occupied in the delivery. In half an hour after the operation the patient's pulse had fallen to 104, and she was left with directions to have an anodyne draught, if restless.

20th. She slept well without opium, has no pain in the abdomen, passed water freely this morning. Pulse eighty. Bowel's confined: to have Ol. Ricini 3j.

This patient had no unpleasant symptom afterwards, and recovered as well, and as speedily, as the other women in her ward.

On reviewing these cases it will appear, that of the seven cases of delivery by the forceps, four of them were head presentations in uncomplicated labour, in three of which the child was saved; two of these being first pregnancies. In the other cases of head presentation, complications arose which usually produce the death of the fœtus, and in the footling case the loss

of the infant must be attributed to the unfortunate delay in procuring the instruments.

It may be objected to the practice above detailed, that in the first four cases delivery *might* have been accomplished by the natural efforts, and therefore instrumental aid was not necessary. True, delivery *might* have been accomplished, but I would ask at what risk would the experiment of waiting be made? Urgent symptoms of local and general irritation had manifested themselves in all, before any attempt was made to assist the uterine effort, and the head of the infant was arrested in such a position as precluded the hope that it could be passed through (if at all) without several hours' severe labour, and a corresponding increase in the dangerous symptoms, which must have arisen to such a height before delivery, as would place the woman's life in immediate danger, and her future comfort in considerable jeopardy; besides in all probability destroying the life of the child. From this state of peril the women were rescued, and three of the children were born alive, which, under other treatment, must have been sacrificed. They were, I say, rescued by a safe operation, and they recovered as well as if their labour had been natural and easy.

With respect to laceration and sloughing of the vagina, bladder, &c., stated by some authors to be caused by the forceps, and used as an argument against their employment, I am of opinion that in the majority of cases, when these lamentable results occur, the blame is unmerited; because I have seen the worst inflammation and sloughings of these parts follow in cases where the perforator had been used, and even in some where no instrument whatever was employed. The truth is, the mischief is effected by the pressure of the infant's head upon the soft parts of the mother, and after this has been continued with sufficient intensity, for a sufficient length of time, the inflammation caused thereby will run its course, no matter in what way delivery is accomplished, whether by the natural efforts, or by instruments. But it

frequently happens that delivery is effected in these cases by instruments, *too late* to prevent the unhappy results alluded to, and then the operation is charged with the consequences. If an accurate account of the subsequent condition of all women after delivery could be obtained, I much fear that the histories of those cases in which labour had been allowed to run too long before interference was used, would be anything but satisfactory. The lamentable sloughings of the vagina, with subsequent closure of the passage by the process of cicatrization; or the still more distressing sloughing of the bladder with its attendant urinary fistula, are seldom mentioned in lying-in hospital reports, because the patients are usually removed from those institutions before such results have become very apparent; and thus, a case left to nature, in which delivery is effected by the natural efforts, is set down as a favourable one, without any notice of its consequences.

That injury may have been effected by unwarrantable roughness in the use of the forceps, or by ill made instruments, I admit, but that injury is even likely to result from the use of this instrument, when judiciously employed, I entirely deny. I have seen a surgeon, in performing the operation of lithotomy, force his forceps through an imperfect opening into the bladder, and then, having grasped the stone, finding that he could not extract it by ordinary force, place his foot upon the cross bar of the table, and pull with all his might, and nearly fall on his back, when the stone came out with a jerk. I have known the patient to die after such a proceeding; but am I on that account to decry the operation, and deny its saving powers, when the instruments are in the hand of a skilful and dexterous surgeon? By no means; I charge the blame where it is due, I decry the bungler; but my faith remains unshaken in the operation, when conducted upon rational and scientific grounds. Whence then proceed the local injuries to which I have alluded? manifestly from delay, after urgent symptoms have set in. This opinion is not advanced for the purpose of recommending rash or hasty

interference in all cases ; (the table of prolonged labour already given shews that such is not my practice) ; but it is with the view of inculcating the obligation to watch our cases closely, and to interfere when necessary, and only then.

This point of necessity is the great question in dispute between practitioners, and I trust there are few at the present day willing to follow the directions of Dr. Osborne, " to wait till the powers of nature are absolutely or altogether exhausted," when, as it has been said by the late Dr. Beatty, " our interference can only remove a dead child from a dying mother." On the contrary, I am happy to believe that the prejudice so long existing against the *timely* use of the forceps is subsiding, and that many practitioners will agree with Dr. Hamilton, " that the great utility of this mechanical contrivance is, that it enables the practitioner to prevent the occurrence of those untoward symptoms, which Dr. Osborne has described as alone warranting the use of the instrument."

CONVULSIONS.

Mary Nolan, aged 20 years, of a full, plethoric habit, first pregnancy, admitted December 20th, 1836.

On admission the os uteri was dilated to the size of a half-crown piece, and the pains were regular and efficient. Labour progressed steadily, and at the end of four hours the dilatation of the os uteri was nearly complete, soon after which the head began to advance into the pelvis, and at the end of six hours the greater portion of it had passed the brim, and nearly filled the hollow of the sacrum. The pains continued to recur with considerable violence, without making much impression on the position of the head, and the external parts being rigid and dry, she was bled to ℥xiv . This had the effect of relaxing the vagina and perinæum, but at the expiration of twenty-four hours, the head, although lower in the pelvis, and moving at each pain, had not yet come to press strongly on the perinæum. In four hours more, (twenty-eight from admission), there was

no untoward symptom calling for interference, the pulse was eighty, the pains regular, and the head manifestly advancing, when she was suddenly seized with a violent convulsion. Delivery by the forceps was immediately had recourse to, and much difficulty was experienced in the operation, owing to the unmanageable state of the woman, and the great size of the infant's head, which was found to measure four inches between the parietal protuberances. The child was dead. The delivery was accomplished at one o'clock, A. M. of the 21st. From that time she had four convulsions, diminishing in severity through the night, but towards morning she sank into a comatose state, from which she could be roused, not however to consciousness, but to mania, her face being flushed, her eyes starting, the pupils contracted, and she resisted with violence any attempt to administer medicine by the mouth. She did not speak, and when allowed to remain undisturbed, her breathing was stertorous, accompanied by a hissing noise in expiration.

21st, Eight o'clock, A. M. Finding the above-mentioned symptoms to have set in and her pulse 140, she was bled to \bar{z} xxiv; and in a few hours the bleeding was repeated to \bar{z} xxx. as the former abstraction had produced no relief. The blood drawn on both occasions was highly buffed and cupped; has had no convulsion through the day.

Six o'clock, P. M. The breathing less laborious; pulse 130; her head was shaved and cold lotion applied.

22nd. Spent a restless night; comatose symptoms still continue, but the violence of the mania is somewhat abated. A bolus of calomel and jalap was got into her mouth, which she swallowed, and an enema with spirit. terebinthinæ having been administered in four hours afterwards, the bowels were well freed.

Six o'clock, P. M. Symptoms as before.

V. S. \bar{z} xviii., and a blister was applied to the head.

23rd. Passed another restless night, the urgent symptoms

of coma rather less, but she is still quite insensible to questions put to her; pulse, 130. Ordered a repetition of the bleeding to sixteen ounces, and a blister between the shoulders.

Six o'clock, P. M. The symptoms have yielded since last report, she can now answer yes and no, there is not much stertor in her breathing, and her pulse has fallen to 118, and is soft; she passes fæces and urine involuntarily.

24th. She had some quiet sleep during the night, and is more collected this morning; complains of the blister on her back; takes her drink; pulse 100.

From this time she continued to improve gradually, but she had no recollection of any part of her labour.

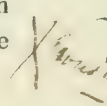
RUPTURED UTERUS.

Anne Gaynor, aged 27 years, an extern patient living in Purdon-street; fifth pregnancy, March 11th, 1837. Labour commenced at seven o'clock in the evening, the waters were soon discharged spontaneously, and the head came down into the pelvis in two hours. The pulse was a little excited, and the countenance florid, the vagina rather dry, but cool. The pains continued smart but ineffectual, communicating very slight impulse to the infant's head, and recurring every ten minutes. The patient was very restless, getting out of bed occasionally and walking about, and when in bed she was continually changing her position. At eleven o'clock there was a discharge of about four or five ounces of blood from the vagina, but there was nothing remarkable in the pain immediately preceding the occurrence. Shortly after, however, her pulse began to alter in character, becoming smaller and quicker; her complexion began to fade, and the pains, in the same gradual manner, declined in degree, but not in frequency, for about two hours, when they ceased entirely. About this time she began to complain of pain in the epigastrium, where the fundus uteri pro-

jected in a remarkably prominent manner. A soft diffused swelling appeared above the pubis, which continued to ascend slowly towards the umbilicus, and expand laterally, masking the uterus as it advanced, by intervening between it and the parietes of the abdomen. Vomiting now set in, and intense thirst, to relieve which she drank large quantities of cold water, but immediately rejected each draught. The sinking of countenance, depression of strength, restlessness, and anxiety, progressed very gradually but steadily from this period, until the pupils in charge of the case (who had neglected to send for assistance to the hospital when alarming symptoms had set in,) reported the condition of the woman. On visiting her she was found to be moribund, the pulse imperceptible, and the extremities cold. The head of the infant being still in the pelvis, it was delivered by the forceps, immediately after which the woman expired.

On introducing the hand, a large laceration was found at the neck of the uterus, immediately in the neighbourhood of the promontory of the sacrum, which was unusually prominent and sharp; and, on passing the hand through the rent, a large quantity of coagulated blood was found in the abdomen among the intestines. Leave could not be obtained to make any further examination of the body.

In this case it is manifest, from the symptoms, that death was the consequence of hæmorrhage into the cavity of the peritonæum. This mode of termination might possibly have been avoided, if timely notice had been given; but the woman, if saved from death from this cause, would still have had to encounter the usual danger of peritonæal inflammation. It is to be remarked, that two symptoms mentioned in books, and often present in cases of ruptured uterus, did not accompany the instance just detailed: namely, a sudden pain and sensation as of something having given way within the patient, and a receding of the presenting parts. The latter was prevented by the low position the head had reached in the pelvis before the laceration took place. The unusual prominence and sharpness of the



promontory of the sacrum, furnish an explanation of the readiness with which rupture took place in the case before us. For, the woman having previously borne children, it is easy to conceive how the neck of the uterus must have been compressed against this sharp ridge, whereby an amount of inflammation, capable of altering its texture, would have been excited, which would render the part thus diseased unable to bear the distention attendant upon a subsequent labour.

RETAINED PLACENTA, FATAL HÆMORRHAGE.

Mary Brophy, an extern patient, living in Fleming's-place ; sixth pregnancy, aged thirty years, October 6th, 1836. This woman was delivered at eight o'clock in the morning, after an easy labour of twelve hours' duration. The placenta was retained until twelve o'clock, when I was summoned to her aid. A continued draining of blood had been going on from the time of delivery, but not being at any time very great, her attendants were deceived as to its danger. I found her in an extreme degree of exhaustion from loss of blood, her pulse imperceptible, extremities cold, countenance pallid, and great restlessness, with jactitation. She complained of oppression of breathing, and of noise of carts rattling in her ear. Strong spirituous stimulants were now administered, and I passed my hand into the uterus, where I found the placenta partially adhering, and the remainder detached from the uterus. I removed it and a large quantity of coagulated blood at the same time, and the uterus was made to contract by friction and strong pressure over the pubis, in which state it was firmly bound down by a broad bandage and compress. Opium, and stimulants, with friction of the limbs with warm flannels, were now liberally employed, but without the effect of rallying the patient. The hæmorrhage was quite arrested, but the constitution seemed to have received too great a shock from the previous loss, to be able to recover, and the woman sank by degrees, and finally expired three hours after the removal of the placenta, and seven from the time of delivery.

This case is an example of the insidious and dangerous nature of that form of hæmorrhage, in which there is no considerable outburst of blood at any one time, but the inexperienced attendant is lulled into security, by the apparently small quantity of blood the patient is losing, not considering that, though small at any one period, yet when allowed to go on for a length of time, the aggregate becomes a serious quantity, and thus the patient's life is run into the greatest danger before alarm is taken.

PUERPERAL FEVER.

The hospital was visited by this terrible malady twice, during the period embraced by the present Report. Both attacks took place in the month of January, and at each time *erisypelas* was raging as an epidemic in the surgical hospitals, and diseases of a typhoid type were very prevalent in this city.

The first case about to be related, is one, which, under other circumstances, might be considered as not being entitled to the name of puerperal fever, because the organs principally affected were not those in which lesion is usually found in this disease, but from its being the first case that occurred at that period, from the typhoid symptoms by which it was attended, and from the severity of the disease in the other women, who were immediately attacked in the same ward, I have little hesitation in ascribing it to the same influence that produces the better marked puerperal fever, although the local affections in this case were different from what are usually present.

CASE I.—Anne Early, aged 22 years, first pregnancy, delivered January 30th, 1836, of a living child, after nine hours of labour; continued well the next day.

February 1st. Complains of acute pain in the left side, under the breast, and extending to the back, which prevents her making a full inspiration, and produces a good deal of dyspnœa. There is some tenderness over the uterus increased on pressure; countenance flushed, pulse 130, small. She was bled to sixteen ounces, cupped on the side, and ordered a laxative draught.

Ten o'clock, P. M. No relief. Breathing forty in the minute ; dyspnœa increased ; pulse 140. Examination with the stethoscope indicated intense bronchitis in both lungs.

Repr. V. S. ad $\text{̄}xvj$.

A pill of calomel gr. ij., and opium $\frac{1}{2}$ gr. to be taken every two hours.

2nd. Had profuse perspiration during the night, which still continues ; symptoms as before ; slight cough, but no expectoration. Tenderness of the belly continues. The calomel was increased to 3 grains, and opium to $\frac{1}{3}$ grain every two hours.

Ten o'clock, P. M. Breathing easier, feels less oppressed, countenance pale ; pulse 130. No appearance of salivation.

Contr. pil. A blister to both sides.

3rd. Slept none ; breathing laboured ; great exhaustion ; countenance sunk ; pulse 130, weak and small ; no expectoration ; intense muco-crepitating rale all over the chest on both sides. Belly more distended and tender. Ordered an emetic of ipecacuan and tart. emetic.

Four o'clock, P. M. Great distress in breathing. Pectoral sounds less audible, chest sounds dull on percussion ; pulse scarcely perceptible.

\mathcal{R} Ammon. Carb. $\text{̄}ss$.

Mist. Camph. $\text{̄}vi$.

Sumat $\text{̄}i$. omni horâ.

From this time she continued to sink, and died early on the morning of the 4th.

Post mortem. A copious effusion of sero purulent fluid was found in both sides of the chest. The fluid resembled closely that usually found in the peritoneum in cases of puerperal fever. Patches of lymph adhered in different places to the serous membrane. The lungs were compressed, dark-coloured, but not hepatised, and when cut into, they gave out a large quantity of frothy sero purulent fluid. There was a small quantity of fluid in the pericardium. The peritoneum exhibited vascularity in different places, but there was no effusion of lymph or fluid into its cavity, neither were there any adhesions.

CASE II.—Jane Beatty, aged 22 years, second pregnancy, delivered at seven o'clock, P. M., February 2nd, 1836.

This woman lay in the bed opposite to her's whose case has just been detailed. A few hours after delivery she had a rigor, and soon complained of severe pain in the belly, accompanied by vomiting, pulse 110. She was leeches freely, and fomented over the belly, and an emollient enema was administered.

3rd. Nine o'clock, A. M. Belly tympanitic; severe pain; vomiting continues; countenance muddy and sunken; pulse 104, weak. Gave her an ounce of spirit of turpentine, which was soon rejected.

Turpentine fomention to the belly.

Pills of calomel and opium 2nda q. q. horâ.

Four o'clock, P. M. All symptoms worse.

Ten o'clock, P. M. Evidently sinking; pulse imperceptible; feet cold; vomiting continues.

4th. At seven o'clock, A. M. she died, just thirty-six hours from the time of delivery.

P. M. seven hours after Death.—The tympanitis of the belly had subsided, and the uterus could be felt as high as the umbilicus. On opening the abdomen the peritoneum lining its walls, and covering the viscera, was found minutely injected with red vessels in every part, but particularly over the uterus, fallopian tubes, and ovaries. There was a small quantity of thin, uniform, reddish-coloured, serous fluid in the cavity, but no lymph. An incision in the long axis of the uterus displayed its walls of great thickness, and its inner surface was of a deep chocolate colour, particularly in the cervix and neighbouring part of the vagina. A small quantity of serum existed in the pericardium, but there was no trace of disease in the pleuræ.

CASE III.—Margaret Mooney, aged 29 years; fourth pregnancy, delivered February 2nd, 1836.

In this case the infant presented with the breech, and was born alive, after a labour of three hours.

4th. Ten o'clock, A. M. Had a shivering fit in the night, complains of pain in the belly, increased on pressure; great thirst; countenance sallow; pulse 120.

V. S. ad. \bar{z} xvi.

Leeches to the abdomen, with fomentation, calomel and opium every second hour.

Ten o'clock, P. M. Belly tympanitic; pain severe; countenance sunk; pulse 130. Continue the pills, two drachms of mercurial ointment to be rubbed into the belly.

5th. Slept well; pain and tenderness of the belly as before; lies on her back; has not passed water for several hours: a catheter was introduced which gave exit to some turbid, high-coloured urine. Continue the pills and ointment.

Ten o'clock, P. M. Belly not so much swollen, passes fæces involuntarily; pulse 128. Sleeps a great deal.

6th. Belly more tympanitic; pulse 140, weak; countenance greatly sunk. Continue pills, with a mixture of carbonate of ammonia.

She continued to sink, and expired at seven o'clock, P. M.

CASES IV. AND V.—Julia Cahill, and Elizabeth Kelly. These two patients were delivered in a ward distant from that occupied by the last three, at one o'clock, A. M., February 5th. They both shivered in the evening of that day, and soon were attacked with pain in the belly. Cahill, who was a strong, plethoric woman, was bled to \bar{z} xvj. Kelly being weak, thin and pale, was not bled, except by leeches to the belly. The disease ran a somewhat longer course than in the other women who died. Cahill died on the 8th, and Kelly lived to the 9th. They had both very early exhibited the sallow, muddy, sunken countenance, a symptom which I have always found to forebode a fatal termination. No post mortem examination was made of these women.

Three other patients were attacked with alarming symptoms, which yielded to treatment similar to that just mentioned in the other cases.

The hospital was now closed against any further admissions. Patients who were sufficiently convalescent after delivery were sent home, and the walls were thoroughly cleansed, painted, and white-washed, after being exposed to the vapours of chlorine for two days. The admission of patients was resumed in a fortnight, and no new case of the disease appeared for twelve months, when in the month of January, 1837, a fresh burst of the disorder took place.

Five women were now attacked, of whom three died; and it is to be remarked that on this occasion also, the first patient in whom the disease exhibited itself, had severe symptoms of thoracic disease similar to those in No. I.

CASE VI.—Mary Finlay, aged twenty-six, fourth pregnancy, delivered January 7th, 1837, after a labour of six hours.

9th. Complained of pain in the right side, with cough and dyspnœa; tenderness over the uterus; pulse 120. She was bled from the arm to ℥xvi. ; and in the evening, finding that the pain and difficulty of breathing continued, and that the stethoscope indicated bronchitis in the right lung, she was cupped on the side, and ordered pills containing Cal. gr. ii. Ipecac. gr. i. and Opium gr. $\frac{1}{4}$, one to be taken every second hour.

10th. Pain in the abdomen has increased and extended over the cavity. Pulmonary distress not relieved; pulse 130, weak. Leeches were applied over the uterus, followed by fomentations; the pills to be continued.

11th. The leeches produced some remission of the abdominal pain, but the difficulty of breathing still continues, and both lungs now appear engaged; the physical phenomena of bronchitis being manifest on both sides of the chest. The pulse, from its weak character, forbid further depletion. Blisters were applied to the sides. No appearance of mercurial action. Pills continued, and a drachm of mercurial ointment put into each axilla.

12th. Was attacked with diarrhœa this morning, which still continues, and has exhausted her very much. The colour of

the skin on the face, neck, and chest, is dingy; pulse 140, feeble; belly tympanitic. Ordered tinct. opii with aromatic confection, and camphor mixture. She continued to sink, and died at ten o'clock, P. M.

On examination after death, considerable effusion of sero-purulent fluid was found in both sides of the chest, and the lungs contained the frothy fluid characteristic of the last stage of bronchitis. In the abdomen the peritoneum was very vascular, and the same kind of fluid as in the chest was found effused. Patches of lymph covered the intestines, and were numerous about the pelvic viscera.

CASE VII.—Alice Kavenagh, aged thirty-three years, delivered of her fourth child, after a labour of fourteen hours, same day as last case. This was a poor, wretched-looking, dejected creature, confined in the same ward with Finlay. She was attacked on the second day after delivery with shivering, to which speedily succeeded acute pain in the uterine region. Pulse 120, small and weak.

Her whole condition contra-indicated general blood-letting. Leeches were applied to the abdomen, and repeated in six hours. Fomentations were used diligently, and she was ordered calomel with opium every second hour. The disease assumed the typhoid type from the first in this patient; she soon put on the leaden hue of countenance, resembling that observed in cholera so strongly, that those who saw her were immediately struck by it. The symptoms progressed in spite of all measures used to arrest them, and she died on the 12th, the fourth day after the seizure.

CASE VIII.—Sophia Cameron, aged twenty-one years, delivered of her second child, January 10th, 1837, after five hours' labour.

This patient came into the hospital under peculiar mental depression. She was attacked on the second day after delivery with severe pain in the belly, which soon occupied the entire region, and she could not bear the slightest pressure in any

part. Pulse 120. She was bled to $\frac{3}{4}$ xvi, and leeches were put on over the whole abdomen. She got a draught of six drachms of castor oil, with the same of spirits of turpentine, which was soon rejected by vomiting. Intense thirst harassed her, and whatever she drank was speedily ejected. Calomel and opium, with mercurial frictions, were freely employed, but without effect. She became delirious on the third day of the attack; and, although her pulse was imperceptible, her strength continued surprisingly great; she was able to sit up occasionally in the bed, and her last act was to make a turn from the right side to the left, when she instantly expired. No examination.

The hospital was again closed, and the same means of purification were pursued as on the former occasion.

The wards were re-opened in a fortnight, after which we had no more of the disease. It appears remarkable, that at both periods of its invasion, the disorder shewed itself first in the thoracic organs. Its occurrence in these two cases was very different from the extension of the disease from the abdomen to the thorax, which is so common in the last stages of ordinary cases, when pain in the former cavity ceases, and oppression of breathing comes on. But, in the cases alluded to, the first symptoms complained of were pain in the side, and dyspnœa, and to these succeeded the usual symptoms of abdominal disease; the former, however, continuing the most prominent throughout.

We had a striking instance of the portability of the disease during the former visitation, in the case of a woman living in Camden-place, a considerable distance from the hospital. This woman took her labour, and sent to the hospital for assistance; she was attended by two of the pupils who had been about the patients then ill, and she was seized with puerperal fever on the second day, and died on the fifth. Being the only patient in that district, who at that period was attended from the hospital, I was anxious to know whether the disease was existing in her neighbourhood; but on careful inquiry, we could not learn that any other woman had been affected by it. From this, it is fair

to conclude that the infection was conveyed to her by her attendants.

It is not usual to believe that persons not puerperal can be influenced by the disease under consideration ; but three cases occurred during the former epidemic, which afford some grounds for thinking that such may be the case. The two nurses who were engaged about the sick, were both attacked with a low form of fever, in which great prostration of strength was a prominent symptom, and from which they recovered very slowly. The unusual exertions and watching which they were called upon to use at the time, might, no doubt, have contributed to produce fever, but it seems at all events, a coincidence worthy of remark. The other case was one which had been under treatment for a considerable time in the Hospital, in consequence of

PARALYSIS AFTER DELIVERY.

Anne Kiernan, aged 21 years, delivered of her first child, November 26th, 1836, after a labour of seven hours ; infant alive. Nothing remarkable occurred during labour, or afterwards, until she complained on the second day, that she could not move her right leg, and that it felt benumbed and dead. On examining the limb, no swelling or pain could be discovered at any part that could indicate the approach of phlegmasia dolens ; on the contrary, the sensibility of the limb appeared considerably lessened.

Frictions, with warm turpentine, were ordered to the limb, but without any effect upon the condition of the part. At the end of a fortnight, finding that no improvement had taken place, a course of blisters along the line of the sciatic nerve was commenced, beginning above, and going downwards. This plan, together with attention to her general health, had the effect of gradually restoring the power of the limb. In a month she was able to walk across the ward with the assistance of a stick, but even yet the leg was dragged along with difficulty, and when

carried forward, the foot hung loose and vacillating, the toes pointing to the ground. In another month she had regained considerable power over the muscles, her progression was much more firm and steady, and the sensibility of the limb was almost entirely restored. She continued to improve until the month of February, at which time she was walking about nearly well, and preparing to leave the hospital, when puerperal fever made its appearance in our wards. I have mentioned that on the invasion of the disease this hospital was closed, and cleared of all patients; but the woman being very poor, and more than two months having elapsed from the time of her confinement, I felt no apprehension in allowing her to remain. On the fifth of February she was seized with alarming symptoms. There was great general uneasiness and depression; the pulse beat 130 in a minute; she complained of pain in the epigastric region, and there appeared to be much embarrassment in the respiration.

The next day all the symptoms were aggravated, and she was removed to Sir Patrick Dun's Hospital, where she died in about a week of pericarditis. The post mortem examination of her body disclosed a striking specimen of the disease, having passed to its third stage. There was a considerable quantity of sero-purulent fluid in the pericardium, and flakes of lymph covered the heart and lined the sac.

Here again, if it was not the effect of contagion, was a striking coincidence of disease. It will be observed, that it was a serous membrane that was engaged, and that it terminated in effusion, similar to that poured out in the peritoneum in many cases of puerperal fever, and that the affection came on at a time when disease, invading other serous membranes, was prevalent in the hospital.

It is not presumed, from a single case like this, to found any positive opinion as to the real origin of the disease, but coupling it with the typhoid form of fever with which the two nurses were affected, I cannot divest my mind of the idea that I

formed at the time, that they were all the result of exposure to the noxious influence of puerperal fever.

ACEPHALOUS MONSTER.

January 20th, 1837. Mary Maguire, aged 25 years, having come to town for advice, was sent to me by a professional friend for my opinion. She states that she is a widow, her husband having died seven months ago, that she was married two years, and had no child, and that the menses have been suppressed ever since the death of her husband. In consequence of this symptom she had been supposed to labour under amenorrhœa, and had been subjected to active treatment in the country for its removal. By degrees her abdomen began to swell, and her feet and legs became œdematous. Dropsy was now supposed to be her disease, and the usual remedies were had recourse to, with no sparing hand. Finding that notwithstanding all she had gone through, the increase in size still continued, she determined to come to town for advice.

A glance at the woman's countenance told me that she was not labouring under any serious disease, for although she had run the gauntlet of repeated bloodletting, mercury, purgatives, and diuretics, she still retained a glow of health in her cheeks quite inconsistent with a grave malady. This I may remark is a circumstance not to be overlooked in judging of obscure cases of pregnancy, when, from ignorance or design, the attendant is likely to be deceived by the statements of the patient.

Her abdomen was very large, and on examining in the recumbent posture, I found a large tumour rising from the pelvis, as high as the umbilicus, uniform in its surface, and of a globular shape. On applying the stethoscope a faint souffle was heard at the lower part on the left side, and a very indistinct pulsation of the foetal heart was distinguished at the right. On passing the finger into the vagina, the cervix uteri was found diminished in length and softened, and inspection of the breasts disclosed well marked areolæ, studded with prominent emi-

nences. From these symptoms I had no hesitation in declaring her to be seven months pregnant, and I advised her to return to the country until the time of delivery drew near, but to take no medicine except some laxative pills with which she was furnished.

On the 20th of March she returned to town, more than ever convinced that she was afflicted with dropsy. The abdomen was now of immense size, and she complained loudly of the weight and inconvenience produced by the water, which could be felt fluctuating and gurgling whenever she moved or changed her position in bed. In addition to the other inconveniences, strangury to a great degree was present; to which may be added severe pains in the loins. No trace of the foetal heart could be now discovered, and she mentioned that she had not at any time felt the motion of the infant. The souffle was audible in the same situation as before. The os uteri was found of a conical shape projecting into the vagina, and dilated to the size of a six pence, through which the membranes could be distinguished. I informed her that labour was about to set in, and she preferring to be confined in her own lodging, I gave her in charge to an advanced and intelligent pupil.

March 21st. True labour commenced at 10 o'clock A. M. and I saw her at 4 o'clock P. M. The os uteri was then completely dilated, and the membranes protruded in a large ball into the vagina; no presentation could be discovered. I ruptured the membranes, when an enormous rush of water took place, deluging the bed, and running out upon the floor, so as to flood all the part in its neighbourhood. The quantity which continued to pour out for a length of time must have been considerably more than a gallon. Presently a solid substance came in contact with the finger, of the nature of which I was at first uncertain, but as the uterus continued to act, and it was forced lower down, I soon distinguished the soft eminence on the upper part of an acephalous foetus; the tops of both ears were now felt projecting above the temporal bones, and the

edge of the imperfect frontal bone could be felt over the orbits. Uterine action came on briskly, and in half an hour she was delivered of the infant, which shewed no sign of life, but was not in any degree putrid. It was very small, measuring only sixteen inches in length, and weighing five pounds. The occipital bone was deficient above the foramen magnum, and the cervical vertebræ were imperfect along the back, leaving the medulla spinalis exposed as far as the dorsal vertebræ. Anteriorly the foetus appeared to have no neck, the skin of the cheeks and chin being continuous with that on the thorax.

The great quantity of liquor amnii that was present in the uterus of this woman, was in accordance with what has been observed in other cases where similar monsters were produced. Why this should be is difficult to account for. We know that small infants are often surrounded by a large quantity of liquor, and *vice versa*,* and in this instance, the foetus being small, the proportion of fluid might have been in obedience to the usual law. But why, when the monster is even above the usual size, (as I have seen,) this disproportion should exist, still remains to be explained.

PROLAPSUS UTERI.

Anne Foster, aged 47 years, admitted September 16th, 1835. The tumour projecting from the vulva is of a conical shape, the base above. The length of the projection four inches and a half on the anterior surface; the breadth of the base, where it issues from the vagina, is three and a half inches, breadth of apex two and a half. The apex was formed by the os uteri, which was rounded, swollen, and covered with a glairy, whitish

* Son abondance est généralement en raison inverse de la vigueur, du volume, de la force du fœtus, et de la constitution robuste de la femme : en sorte qu'un fœtus de cinq livres, par exemple, naîtra dans deux, trois, ou quatre livres d'eau, tandis qu'on n'en trouve qu'une livre autour d'un enfant de huit à neuf livres.—VIEUVEAU, *Traité de l'Art d'accoucher*, p. 161. Brussels Edition.

matter, through which the surface could be seen excoriated. The inverted vagina was tense, smooth, and very red, and the whole tumour was very painful on being touched. A portion of the urinary bladder had been carried down with the uterus, which was made manifest by passing a catheter into the urethra, when the instrument passed downwards. She states that the prolapse occurred suddenly, three months ago, when she was making some violent exertion, and that it has never been replaced since. No attempt at reduction of the tumour was made at first; but the patient was confined to the horizontal posture in bed; warm fomentations were applied to the part, and the bowels were freed by mild laxatives.

21st. The heat, tension, and redness of the tumour are much diminished.

22nd. Having smeared the surface with oil, I proceeded to reduce the prolapsed organs. After some minutes' slow and steady pressure, the bulk of the tumour began to diminish, and at length passed up with a jerk. After reduction, the os and cervix uteri felt greatly enlarged, and the woman complained of pain in the pelvis. The horizontal posture was still enjoined, and injections of warm water were thrown into the vagina every fourth hour.

24th. The size of the cervix uteri diminished one-half; she can pass water without any descent of the uterus.

26th. Tumefaction of the os uteri gone; parts feel natural; she has no pain, and feels no sensation of prolapse on standing up.

October 12th. She left the hospital well, without any mechanical support to the pelvic viscera.

15th. Came to hospital to report that she is at work as usual, and feels no return of her former complaint.

The cure which seems to have taken place in this case is, I think, owing to the recent and sudden occurrence of the prolapse, and the inflamed condition in which the vagina was found. The first circumstance enabled the organs to resume their posi-

tions in the pelvis, when replaced, with more facility than when a longer time has been suffered to elapse between the displacement and the restoration. And the second enabled the vagina to become fixed in its situation, when brought in contact with the parts from which it had been forcibly detached.

HYDROCELE OF THE NECK.

In the first Report of the hospital, published in the Dublin Medical Journal, September, 1835, the following notice occurs:—"An example of that rare disease, hydrocele of the neck, presented itself in one of the infants. It was not observed until about a week after it was born, and was then about the size of a nut, situated immediately below the middle of the left clavicle. It has continued to grow rapidly, and is now, at the end of six months, as large as a small orange. The coats are thin, and diaphanous, the sac not tense, and no pain appears to be felt in it. I was unwilling to interfere with it during the infancy of the child; but I propose passing a seton through it at some future period."

The foregoing note of the case was taken in June, and on the 11th of September, 1835, the child was again brought to the hospital. He is now ten months old. The tumour has increased in size, and is more tense.

A small trochar was passed into the under part of the tumour, and a clear, sherry-coloured fluid was allowed to flow out. Not intending to pass the seton this day, I withdrew the canula after about half the contents had been evacuated, in the hope that the tumour would have been diminished on the following day.

12th. The child was attacked with severe diarrhœa in the night. The tumour is as large as before the operation. Medicine was prescribed for the intestinal affection.

14th. The child is now well. The trochar was again introduced, and fluid of a lighter colour than before escaped. When

the sac was half empty, I withdrew the canula, and passed a probe, armed with a cotton seton, into the aperture. The point of the probe being pushed to the upper part of the tumour, and made prominent, I cut down upon it and drew the probe through, leaving the seton behind.

15th. Passed a restless night. Tumour larger than before the operation. The seton blocks up the opening, so that no fluid escapes. A probe passed by the side of the seton gave exit to a quantity of fluid, paler than what had formerly been discharged. This coagulated quickly on cooling in the vessel in which it was received.

16th. The child had another restless night ; tumour same size as yesterday, but it feels harder, its parietes being thicker ; the fluid is muddy, and again coagulates but not so completely.

17th. Passed a good night ; tumour very hard, hot, and painful ; the apertures through which the seton passes are suppurating ; fluid very scanty. Considering that a sufficient degree of irritation was now established in the sac, and not wishing to excite too much inflammation in it, I withdrew the seton.

18th. Tumour as yesterday ; fluid oozes from the lower opening of a thin purulent character, and very foetid odour.

19th. Size of tumour not diminished, hardness increased ; it now feels completely solid ; a small portion of thin purulent fluid escapes.

23rd. Diminution in tumour, great hardness ; fluid very scanty ; child sleeps well.

October 1st. The discharge has ceased. The size of the tumour is now about half. The child evinces no pain when it is handled.

From this time it continued to decline daily, and on the 14th of the month scarcely any trace of it could be felt.

Having now mentioned such cases as appeared deserving of notice, I will close this Report by giving a general table of

patients treated in the hospital since its foundation in April 1834.

Total number of women delivered, commencing April, 1834, and terminating August, 1837, *one thousand one hundred and eighty-two*.*

PRESENTATIONS.		DURATION OF LABOUR.	
Head, . . .	1104	Under 6 hours, . . .	557
Face, . . .	4 or 1 in 295 $\frac{1}{2}$	Above 6 and under 12 .	381
Breech, . . .	25 or 1 in 47 $\frac{7}{5}$	12 . . . 24 .	155
Inf. Extremity, 15 or 1 in	79	24 . . . 36 .	43
Sup ^r . extremity, 5 or 1 in	236 $\frac{2}{5}$	36 . . . 48 .	17
Placenta, . . .	4 or 1 in 295 $\frac{1}{2}$	48 . . . 60 .	4
Funis, . . .	6 or 1 in 197	60 . . . 75 .	2
Head and hand, 1		75 . . . 96 .	2
Twin cases, 18 or 1 in	65 $\frac{3}{4}$	96 . . . 136 .	1
Total, 1182		Total, 1182	

PRESENTATION IN TWIN CASES.		FATE OF CHILDREN IN TWIN CASES.	
Both, head in	10	Both alive in	13
Both, feet	1	Both dead	3
Both, breech	1	One alive, one dead . .	2
First head, second feet .	4		
First head, second breech .	2	Total, 18	
Total, 18			

Total number of children born, 1200.

Males, 614, of whom alive, 558, dead, 56

Females, 586, . . . 553, . 33

Total, 1200

Total, 1111 Total, 89

* This table includes those of the former Report.

Of the 89 children still born, there were—

Footling cases, 10	Forceps cases, 3
Breech, 12	Perforator, 5
Funis, 4	Premature, 12
Twins, 8	Putrid, 8
Arm and turning, 6	Natural, 19
Ruptured uterus, 1	Acephalous monster, . . . 1

MOTHERS DIED.

Of puerperal fever,	11
Hæmorrhage after delivery, .	1
Ruptured uterus,	1
Abscess of the ovary,	2
Inflammation of the uterus, .	1
Pneumonia,	1
<hr/>	
Total, 17	

A table, shewing the fate of mothers and children, in cases where the labour exceeded twenty-four hours, has been already given in the present Report.

BIBLIOGRAPHIC NOTICES.

Reply to Mr. Phillips's Remarks on Mr. Aldridge's Criticism on the London Pharmacopœia. By DR. ALDRIDGE, Lecturer on Natural History to the Digges-street Medico-Chirurgical School.

IN the number of the Dublin Journal, published last March, appeared a review of the New London Pharmacopœia, signed by me; and in which I stated honestly my conviction that the work in question was not calculated to supply the wants of the medical profession. The grounds upon which I based my disapprobation of this publication were, 1st., that the list of drugs was incomplete, no description having been given of the varieties proper to be kept in apothecaries' shops, sufficient to enable them to be easily recognized, their purity to be tested, and their adulterations if possible removed: "thus aloes, sarsaparilla, opium, &c., are mentioned, without any clue being afforded to the kind which ought to be preferred; although it is notorious that the varieties met with in commerce differ in the most remarkable degree." And that instead of this useful and indispensable object having been fulfilled, we find the Pharmacopœia made "an arena for the display of learning in deciding difficult questions in natural history and medical literature." 2ndly, I asserted that "with respect to pharmaceutical preparations, this book was miserably deficient;" and I proved this assertion by the quotation of numerous gross and undeniable examples of negligence and ignorance in the directions for preparing syrups, ointments, pill masses, &c. Now, inasmuch as "a Pharmacopœia should consist of an enumeration of the simples which ought to be kept in the laboratories of compounding chemists, together with a formulary for the preparation of all those compounds which are not usually found in commerce,

and which are commonly prescribed by physicians;" and as in the present work both of these important objects are very incompletely fulfilled; I felt myself, as a reviewer, justified in coming to the conclusion, that the publication in question was not calculated to supply the wants of the medical profession. These were the broad grounds upon which I objected to the London Pharmacopœia: the work might possess, abstractedly, much chemical and botanical merit; might evidence much scientific research; but as a Pharmacopœia, it was weighed in the balance and found wanting.

During the investigation of the points above alluded to, it was my duty to animadvert on that "gross perversion of power assumed by the framers of all Pharmacopœias, in dictating to chemical manufacturers, the method and the materials they are to use in preparing things, about which they must necessarily know most." And I brought forward examples to illustrate what the united voice of the medical profession will support me in asserting; "that the chemical processes given in this Pharmacopœia, are in the first place useless and uncalled for; in the second, neither calculated in many instances to obtain the cheapest or purest products." Mr. Phillips, a gentleman of very high chemical celebrity, to whom the construction of the Pharmacopœia was principally intrusted by the London College, has, it appears, taken umbrage at my objections, and has in the last number of this Journal published remarks on my criticism, "relating chiefly to the chemical portions:" to these remarks it is the object of this notice principally to reply.

Mr. Phillips does not state clearly what design he wishes to accomplish by the publication of these remarks. He does not by any means enter into the general question, whether the London Pharmacopœia has fulfilled the expectations, which the medical profession had a right to entertain; he does not attempt to reply to the arguments which I have just stated, founded upon common sense, and independent of details; he has alone noticed certain chemical blunders, which he alleges that I have committed in the examination of particular processes; and appears to deduce from this, that a person so ignorant of chemical science as he represents, was unfit for ascertaining the practical utility of a code of pharmaceutic legislation. Now I think it will not be difficult to prove the insufficiency of this line of argument, even supposing its correctness. I have already stated, that a Pharmacopœia is not intended "as a means of advancing chemical or botanical science; it is for the practical

and useful purpose of requiring the employment of pure and constant medicines;" and chemicals would have been prepared pure by the manufacturer, if the Pharmacopœia had never existed, and will continue to be made so with very little respect for its formulæ. There is no absolute necessity for the medical practitioner to be acquainted practically with the preparation of calomel, more than there is for his having actually inspected the extraction of hepatic aloes. All that is indispensable for him in either case, is to be provided with the means of ascertaining, the efficiency of the agents which he employs in the cure of disease; and, when he refers for information to that work, which he should consider as the standard, and finds it deficient in what he wants, but overloaded with that which is unnecessary, he has an equal right to reprehend the over-legislation which has done too much, as the negligence that has done too little.

But Mr. Phillips has committed himself still farther, supposing this to be his argument; for taking it, during a moment, for granted, that I am unacquainted with those practical minutiae which might enable me to perform the processes recommended by the Pharmacopœia, are not most medical practitioners similarly circumstanced? And, does it not follow, that the persons best calculated to make chemicals, are the manufacturers who have devoted their lives to such preparations? and to whom a variety of circumstances may render convenient, processes the most different. Thus Mr. Phillips has, unfortunately for himself, proved too much; for he has proved the very proposition laid down in my review, "That well-informed chemists, having recourse to the best authorities, are independent of the Pharmacopœia, while, for ignorant persons, its directions are quite insufficient."

Having thus shewn that the justice or injustice of my review is perfectly unconnected with the strictures advanced by Mr. Phillips, I shall now examine the correctness of his remarks.

Some of these remarks are so frivolous and unimportant, that the only reason I can assign for Mr. Phillips occupying so much paper with them was, the absence of more serious objects of reprehension; thus the translating "*nitric fluidunciam*" by half an ounce of nitric acid, fills a long paragraph of censure. Such an error can scarcely be considered of much importance in a review; and, previously to Mr. Phillips noticing it, he should have recollected, that he himself found it necessary to publish a distinct *erratum*, some time after the appearance of the Pharmacopœia, containing the correction of errors the most flagrant and dangerous. Again, we find Mr. Phillips making himself merry at the supposed blunder of stating, "pure silver to con-

tain gold and copper." But his facetious humour would not have presented a very palpable object, had he quoted the passage correctly; "the pure silver of the jewellers" was the phraseology which I used.

Many of these remarks exhibit want of candour, and misrepresentation. I proved the directions of the Pharmacopœia to be insufficient for the production of saleable nitrate of silver, by a detail of those precautions which are necessary for preparing it, and which are omitted in the pharmacopœial account; this detail Mr. Phillips characterizes as a "long rigmarole," although he must be aware of its great practical importance. Again, I objected to the test given for the purity of nitrate of silver, inasmuch as sufficiently full directions are not given for insuring its accuracy. In this I am supported by the authority of Rose, who says, (Griffin's Translation, p. 115,) "It is more accurate to precipitate silver by muriatic acid, than by an alkaline chloride, for the latter is capable of retaining traces of chloride of silver in solution." Mr. Phillips is obliged to admit the charge, yet, with his usual want of candour, he stigmatizes it as a "foul misrepresentation." I asserted that Acetic Acid is for the most part used as a powerful corrosive, while that ordered by the College is too weak to be of the slightest use in medical practice. Mr. Phillips says that this "is an assertion directly opposed to facts, the acetic acid is used in preparing potassæ acetæ, plumbi acetæ, and oxymel." Here is another example of misrepresentation. With respect to iodide of potassium, the Pharmacopœia distinctly states, "Igne subjecto, nihil ponderis amittit." I mentioned in my review that this was "a fact with which chemists were hitherto unacquainted." Mr. Phillips replies,

"With this critic every word is to be twisted as much as possible, in order that it may not mean what was intended; when it is stated that iodide of potassium loses nothing when subjected to heat, according to the critic's version, the heat is to be of that degree which is well known to volatilize it."

How is the Profession to understand either Mr. Phillips's meaning or intentions, except by the expressions which he employs? These are not, by any means, all the examples which I might bring forward, of the total want of ingenuousness displayed by Mr. Phillips in his "Remarks." It would have been more honourable for him either to have confessed his errors, or to have remained silent; such a course would have been more consistent with what is throughout these remarks by no means manifest,—a love of truth.

Having thus shewn that some of Mr. Phillips's remarks are

frivolous, and that many are uncandid efforts to palliate what is confessedly erroneous, I shall examine, in detail, the remaining preparations which he has volunteered to defend. The first preparation which I shall bring before the notice of the reader, is the *Ferri Potassio-Tartaras* of the new *Pharmacopœia*, a direct combination of bitartrate of potash with peroxide of iron, which Mr. Phillips asserts to be identical in composition with the "*Ferrum Tartarizatum*," of the old *Pharmacopœia*. His assertions with respect to this preparation are so singular, that I shall quote his own words :

"The critic says, that in preparing what he calls the old tartar of iron by exposing iron to the action of bitartrate of potash, water, and air, the iron, influenced by induction from the potassium, only acquires a proto-state of oxydation. From this statement it is evident that the critic is ignorant of the fact, that when iron is acted upon by bitartrate of potash and water, the oxygen, which the metal first acquires, is derived from the decomposition of water, the hydrogen of which is evolved in the state of gas."

I feel repugnance, I confess, in being obliged to comment upon this passage, for I am forced by it to infer, either that Mr. Phillips is unacquainted with the meaning of the word "induction," or that he intentionally endeavours to mislead his readers. By this term is simply understood the influence which a body, charged with any kind of electricity, exerts over another, either charging it with the same kind, if it be previously neutral, or diminishing its excitement, if it be before charged with the same kind in a minor degree. It is a necessary result of the characteristic attractions and repulsions, which the electrical fluids exercise towards each other. Yet, Mr. Phillips pretends to discover in my assertion, "that the positive potassium exerts an induction over the less positive iron," an affirmation of the source from whence the latter obtains its oxygen—a most logical conclusion truly. But, that this induction, which Mr. Phillips does not appear to understand, is actually exerted, can be proved by the following considerations. Iron will not rust when partially immersed in a solution of carbonate of potash : what is the cause of this effect, if it be not produced by the presence of the positive potassium, destroying the electrical energy of the iron ? Again, one part of iron, partially immersed in six parts of cream of tartar, moistened with water, (these being nearly the atomic proportions,) suffers a very slow and imperfect oxydation, and that which is acted on will be found mixed with the bitartrate of potash in the form of a white sparingly soluble proto-tartar. Lastly, if you place some coils of iron turnings in a mixture of water and freshly prepared proto-tartar of iron, in the course of

a few hours a proportional quantity of the latter will dissolve, forming a very dark-coloured solution, which will present all the characters of pertartar of iron. In the latter case, the excess of iron overcomes the inductive energy of the potassium, and some slips of copper will be found to act in a similar manner. Here we have presented to us a very beautiful play of mutual induction, with which, in charity to Mr. Phillips, I am willing to believe he is utterly unacquainted.

But Mr. Phillips further remarks,

“It is well known that tartarized iron contains the sesquioxide and not the protoxide, the additional portion of oxygen being acquired from the air.”

So far from its being “well known,” I am not aware of any eminent chemist, always excepting Mr. Phillips, who has expressed such an opinion. Berzelius and Dumas mention two combinations of tartrate of iron with tartrate of potash; in the one the iron being in the state of protoxide, in the other that of peroxide; but neither answering to the characters of that prescribed by the former Pharmacopœia. The proto-tartar of iron is sparingly soluble in water, and precipitates the proto-carbonate when boiled with carbonate of potash;* the pertartar is perfectly soluble in water, and throws down the peroxide, when boiled with carbonate of potash; but, if we examine the “*Ferrum Tartarizatum*” of the Pharmacopœia of 1824, we will find that one-half dissolves readily in water, while the remainder is very sparingly soluble, throwing down peroxide when boiled with carbonate of potash. It will be at once seen, that this preparation is not identical with either of the described tartars of iron. In fact, any one may ascertain that this substance is a mixture of two compounds, quite distinct from each other. I have already mentioned, that it is only partially soluble in water, a fact which is very easily perceived during its preparation; one part of iron turnings and two parts of cream of tartar, moistened with water, and exposed to the air for twenty days, form a dark-coloured mass, which when washed with a sufficient quantity of water, and strained, separates, upon being allowed to stand, into a precipitate, answering to all the characters of proto-tartar of iron; and a solution, the colour of molasses, evidently containing a tartar of the sesquioxide. If without permitting the separation of the sediment, we boil the freshly strained liquid, a copious green precipitate, amounting to upwards of one-half the solid material used, is seen rapidly to form, and this, when separated, presents

* I find that an excess of tartrate of potash prevents this precipitation.

very curious properties. It is very sparingly soluble in water, and yet throws down peroxide of iron, when boiled with carbonate of potash. We are here presented with a union of the properties, separately possessed by the described tartars of iron, and what more distinctly points out its composition is, that when pure proto-tartar, in excess, is boiled with a solution of tartar of the sesquioxide, this green sediment is thrown down, and the supernatant liquid is left completely colourless. It is to be remarked, that this green sediment, which we have just seen to be a combination of proto-tartar and pertartar, is found in much greater proportion in the tartar of iron of the Dublin Pharmacopœia, where a much smaller quantity of iron is employed; a fact to be expected from the experiment already mentioned, in which the presence of an excess of iron hastened remarkably the oxydation of the proto-tartar.*

The reader will excuse these details, not only because they expose the unwarranted assumption of Mr. Phillips's assertions, but, likewise, that they disclose much that is interesting with respect to a preparation which has been hitherto but imperfectly understood. In conclusion, I may remark, that Mr. Phillips has exhibited great negligence, or an intention to deceive, in quoting Dr. Barker as an authority in favour of his opinion. In page 254 of his Translation, Dr. Barker distinctly states, "In this process, a triple salt, the tartrate of potash and iron, is formed; the atom of tartaric acid, which gives acidity to the tartar, being neutralized by protoxide of iron."

The next remark of Mr. Phillips, which I shall bring before the attention of the reader, is one which he has hazarded with respect to my criticism on the process for Antimonii Potassio-Tartras. Having mentioned that tartar emetic is made by boiling cream of tartar with crocus of antimony, I quoted a passage from Berzelius condemnatory of this method of preparation. Mr. Phillips meets this by saying, that the quotation from Berzelius,

"Has nothing to do with the process; for I assert," he continues, "in direct contradiction to the critic, that crocus of antimony is not used by the College. Crocus of antimony is prepared by mixing, deflagrating, and fluxing a mixture of equal weights of sesquisulphuret of antimony and nitrate of potash; and, the crocus is separated mechanically from the saline matter. In the College process, the same ingredients are employed: mixed, however, with some hydrochloric

* It is also to be observed, how completely the fact mentioned in the preceding note accounts for the non-precipitation of proto-carbonate of iron, when the sediment was boiled with carbonate of potash.

acid, this mixture is merely deflagrated, and the saline admixture is separated by washing."

Now, this certainly is a very strange method of arguing; so, forsooth, the identity of a compound depends upon the mode of obtaining, and not upon its composition; either sublimed or precipitated proto-chloride of mercury cannot, therefore, be calomel. Carbonic acid, obtained by the action of an acid on chalk, cannot be the same as that got by burning the diamond. I wonder Mr. Phillips did not think of this method of reasoning, when he so vehemently insisted on the identity of the new and old tartars of iron. But Berzelius' ideas of chemical identity are very much at variance with Mr. Phillips's. He mentions several methods of obtaining the crocus; amongst the rest, partially dissolving the sulphuret in muriatic acid, then throwing into water, when the oxide, formed by the decomposition of the chloride, uniting with the remaining sulphuret, forms "crocus." Surely Mr. Phillips must have forgotten Proust's "washed crocus of antimony," when he mentioned washing as a distinctive character of the College preparation. To examine how far the presence of hydrochloric acid interfered with this product, I took 120 grains of the material, deflagrated according to the College directions, and having boiled it in distilled water, found that 63.5 grains were dissolved; consisting of 60.5 grains of mixed chloride and sulphuret of potassium, and sulphate of potash, with three grains of kermes and golden sulphuret. The insoluble residue was of a saffron-brown colour, and amounted to 56.5 grains, from whence twenty-three grains of sesquioxide was extracted, by boiling with an excess of bitartrate of potash, the remainder was soluble, with the exception of a small quantity of blackish powder in hydrochloric acid, evolving, during its solution, sulphurated hydrogen gas. It evidently possessed the composition of crocus of antimony, with some excess of sesquioxide; and, if Mr. Phillips entertained any doubt as to the accuracy of my experiment, he is at liberty to refer to page 190 of his own Translation, where he will find the following paragraph:

"By the present formula, an oxysulphuret is obtained similar, in composition, to the *Crocus Antimonii* of the *Pharmacopœia* of 1788."

It is plain that Mr. Phillips's only reason for having recourse to so ridiculous a quibble is to shield himself from an objection, that he knew he had no means of honestly refuting.

In answer to my objections urged against Baup's process, for obtaining iodide of potassium, Mr. Phillips says:

"I assert that there is no danger of excess of potash, when due care is taken; and, as to the precipitate of peroxide of iron, of which the critic talks about, there is none; and, so deplorably ignorant is he of the facts of the case, as to suppose, that when proto-iodide of iron is decomposed by carbonate of potash, the precipitate obtained is a sesquioxide, instead of proto-carbonate."

It is astonishing with what facility Mr. Phillips changes his opinions, according as the convenience of the moment demands; the precipitate, in this instance, requires as much washing and exposure, as that which he calls "sesquioxide of iron;" and which, in another place, he asserts not to contain one per cent. of proto-carbonate; yet, in the present case, without any assigned reason, he will not allow the production of any sesquioxide. But let us compare Mr. Phillips's assertions with those of Berzelius, who says, "*Il est difficile, d'exécuter cette opération, de manière à éviter la présence, d'un excès de potasse.*" And again, "*cette perte provient, soit de la filtration, et du lavage, soit de ce que le fer passe à un plus haut degré d'oxidation se précipite dans cet état, et entraîne avec lui une certaine quantité d'iode.*"*

I asserted in my review that pure nitrate of silver, which has never received organic contact, will not become blackened in the sunshine: and I now repeat that assertion, in which I am borne out by the experience of every person who has ever been engaged in its manufacture. "If so, considerable pains have been taken," says Mr. Phillips, "by the authors of the Dublin Pharmacopœia to prevent the occurrence of what never happens." But here again I must join issue with Mr. Phillips; for although pure nitrate of silver remains unaffected in the light, that which has been rolled in paper, and collected particles of dust, will very soon become dark upon exposure.

The next of Mr. Phillips's remarks which I shall refer to are those on the protoxide of mercury. When after taking all possible advantage of a typographical error, whereby I was made to ascribe a much greater impurity of proto-chloride of mercury than I had intended, he proceeds to say, that he had prepared protoxide of mercury by decomposing calomel with lime water. "And in the mode," he continues, "I presume the critic would have adopted, had he performed it—that of *attempting a failure.*" He then details an experiment, wherein, after having saturated the excess of lime in the supernatant liquid by nitric acid, he detected as much chlorine as the calomel could have theoretically yielded. Now, as Mr. Phillips was, according to his own account, "attempting a failure," it was easy for him to employ

* *Traité de Chimie*, traduit par M. Esslinger. T. iii. p. 375.

commercial nitric acid, by means of which he could insure a supernatural quantity of chlorine. But if he means to assert that the protoxide of mercury made in this way is pure, he must have changed his mind very much since the publication of the last edition of his Translation of the former Pharmacopœia, where he says, in page 22 :—"As usually prepared, it is a mixture of calomel and protoxide, or calomel and peroxide—and sometimes of calomel and both oxides of mercury."

The last of Mr. Phillips's remarks which I shall notice is the following :—

"I assert that the iodide of mercury, prepared as directed (by the College), was so perfect, that sulphurated hydrogen, added to the solution of common salt, on which it was triturated, gave not the slightest precipitate."

Mr. Phillips must certainly have been "attempting a failure" when he made this experiment. Biniodide of mercury, when quite dry, is very sparingly soluble in a cold solution of common salt. But if he had boiled some iodide of mercury with a solution of chloride of sodium, he would have found, as I did, an abundant precipitation produced by sulphurated hydrogen. Mr. Phillips may ascribe this to the formation of biniodide during ebullition—and I am quite willing to allow himself to decide which is the more imperfect, his test or his preparation.

I have in the preceding pages endeavoured to reply to Mr. Phillips's remarks ; I have endeavoured to prove that those remarks were quite unconnected with the justice or injustice of my review ; that possibly every objection urged in that review against the chemical preparations of the Pharmacopœia might be erroneous, and at the same time the correctness of my arguments and conclusions remain unaffected, that the Pharmacopœia might contain the best chemical formulæ which could be invented, and yet not supply the wants of the profession. Secondly, I have shown some of those remarks to be frivolous, many to be uncandid efforts to palliate what is confessedly erroneous, and all to be replete with deception, misrepresentation, and error. And thus having shown Mr. Phillips's remarks to be, on the one hand, untenable, and on the other irrelevant, I leave it to the judgment of the reader to decide whether the statements in my review are proved to be "contemptible," and originating "either in inveterate ignorance, or wilful and malignant misrepresentation."

That Mr. Phillips should take considerable interest in the success of the London Pharmacopœia need not surprise the reader, when it is recollected that, independent of his scientific fame, which is most materially involved in a publication, almost exclusively surrendered by the College of Physicians to his care—so direct is his interest in the work itself, that the

publishers considered themselves entitled to prohibit any other translation but what might be executed by him. But whilst we acquit Mr. Phillips of any Quixotic interference in the concerns of others, what opinion are we to form of that corporation, which has surrendered the conduct of such a work to a gentleman, who, whatever may be his scientific attainments, is not even a member of the medical profession.

Before I conclude, I must express my regret at the tone which Mr. Phillips has given to this controversy. It can scarcely be considered consistent with gentlemanly feeling or philosophical dignity to indulge in invective or vituperation. Armed strong in right, I have found no necessity for such weapons; and it is generally considered to betoken great debility in argument, where means so vile must necessarily be had recourse to. Chemistry, perhaps, of all the sciences, is that which most frequently engenders angry feeling. Mathematics, the science of calculation, elevates its votaries above all personal contention. Natural history, the science of observation, encourages a harmony amongst fellow-men, similar to what is perceived in the great creation. But chemistry, depending on experiment for its study, identifies itself so much with our self-esteem and ambition, that envy, hatred, and all uncharitableness spring too frequently from its pursuit. The reader will remember it was in the *Alchemist* Ben Johnson portrayed the *Angry Boy*—a character which Mr. Phillips appears anxious to rehearse. I can assure him, however, of the most perfect good-humour on my part, as long as his scurrility is directed merely towards myself.

But I cannot, with equal complacency, regard the supercilious manner with which he has treated my friend, Mr. Ferguson. I have thought it unnecessary to defend the processes which I have mentioned on this gentleman's authority, partly because in doing so I should forego his valuable assistance, as he is at present absent from this country, and if requisite he is well able to take his own part, and partly because they were not connected with the strictures which I thought proper to pass on the College preparations. But, in answer to Mr. Phillips's unwarranted attack, I may be permitted to say, that Mr. Ferguson is a gentleman, who has already proved his talents by some admirable chemical papers in this Journal—one of which, on the "*Ceratum Saponis*," is probably the occult cause of his present vituperation. He is not, however, a man of one idea—a pure chemist; but possesses extensive mathematical and literary information—a man whose kindly disposition will always excite esteem, while his high moral principle will command respect. Such a man may always despise the hireling pen of a literary "*Pistol*."

The Cyclopædia of Anatomy and Physiology. Edited by ROBERT TODD, M.D., Professor of Physiology, and General and Morbid Anatomy, in King's College, London, &c.

WE have read with pleasure the twelfth part of this valuable and interesting work, which fully supports the character obtained for it by the former ones. Contrary to the usual course of works of this kind, we can discover no falling off, but if possible an improvement accompanying each successive number, entitling it to the good opinion and a favourable reception from the medical public.

Always cautious of bestowing praise, through a fear of misguiding those, with whom our opinions may have some weight, we have refrained hitherto from expressing any opinion with regard to this work, until its own merits, and its uniform and steady progress, call for our acknowledgments. We rejoice to find that its Editor has combined so much appropriate talent in its construction, and we cannot but feel pleasure in knowing, that a mind capable of nice discrimination should be placed at the head of an attempt, which, although it has succeeded in other countries, was looked at with very dubious eye at its commencement in this.

The present part is excellent, although it does not boast any of the distinguished foreign names, which have shed a lustre upon some of the former numbers; yet, it yields in value and the goodness of its articles to none, and the illustrations are admirably executed. It contains the conclusion of Dr. Alcock's paper on the Fifth Pair of Nerves. Abnormal Anatomy of the Fœtus, by Dr. Montgomery. Bones and Joints of the Foot, by Dr. Todd. Its abnormal Conditions, by A. T. S. Dodd, Esq. Muscles and Regions of same, by same. Forearm, Muscles and Regions, by S. Solly, Esq. Fourth Pair of Nerves, by Dr. Alcock. Ganglion, by R. D. Grainger, Esq.

If this work be continued to the conclusion (of which we have no doubt) as it has been conducted hitherto, it will form a most useful standard of reference on all subjects connected with anatomy and physiology.

In our next number we propose to notice Mr. Harrisson's admirable paper on the Bladder, and Dr. Jacob's excellent article on the Eye.

SCIENTIFIC INTELLIGENCE.

Extracts from the Proceedings of the Medical Section of the British Association.—A demonstration or lecture, by Dr. Mackintosh, on the Pathology and Treatment of Dysmenorrhæa, was listened to by a large audience, with excessive interest. As the non-professional journals could not, of course, give any detailed account of this demonstration, we shall be somewhat more particular than usual on this subject. Dr. M. laid on the table at least a score of uteri, all presenting morbid contraction or stricture of the os uteri, in a greater or less degree. Some of them were all but closed—some would scarcely admit a pig's bristle, and all were abnormally contracted. With the history of most of the cases he had been acquainted, and they all laboured under painful, and, generally speaking, scanty menstruation. This contraction of a natural aperture, he regarded as the result of some inflammatory action in the parts themselves, or in their vicinity, more frequently than of any other cause. The straining of the uterus to get rid of the secreted menses, might explain all or most of the phenomena of dysmenorrhæa. The treatment, independently of constitutional means, consisted chiefly in removing inflammation, by the application of leeches (through the medium of a tube) to the os uteri itself—and where chronic contraction obtained, to introduce metallic bougies till proper dilatation was procured. He related several most important and interesting cases in illustration. The patients were chiefly dispensary patients; but afterwards he began to have private practice in the complaint, especially among the wives, daughters, and relations of medical men. It was objected that such operations would not be submitted to by English females. But Dr. M. triumphantly replied that he never *urged*, seldom indeed *proposed* the operation. He explained to the parties the nature of the disorder, and the remedy which he had sometimes found successful; but recommended the patients to try all other means, and, if they pleased, other practitioners, for the cure of their maladies. After a time, the patients themselves, or their friends, would propose the operation, which he always refused to do, till ardently solicited to do so. In this way he steered clear of all scandal.

The second Report of the Sub-Committee, appointed by the Association, to investigate the Motions and Sounds of the Heart, was read by Dr. Charles Williams.

Before describing their last investigations, the Committee stated that they had found frequent opportunities of confirming the conclusions of their former researches on the natural sounds of the heart; and, these conclusions not having been shaken by any subsequent experiment, or well-founded objection, the Committee consider them established: viz., that the first sound of the heart is *essentially* caused by the sudden and forcible tightening of the muscular fibres of the ventricles when they contract; and that the second sound essentially depends on the reaction of the arterial columns of blood, on the semilunar valves of the arterial orifices, at the moment of the ventricular diastole. Certain other circumstances were stated, as being capable of adding to, or modifying these sounds.

The chief subjects of their present inquiry were, the unnatural, or morbid sounds, sometimes heard in the heart and arteries; and, in investigating the causes of these sounds, which Laennec compared to blowing, filing, sawing, purring and cooing, or musical sounds, they sought to determine, 1st, What is the essential physical cause of these sounds; and, 2nd, In what manner disease can develop this physical cause: a correct answer to these inquiries would determine the value of these sounds as signs of disease.

The Committee found that they could produce the same sounds in every variety, by impelling, in various modes and degrees, a current of water through India-rubber tubes; and, by numerous experiments, they ascertained the relations which the character of these sounds bore to the nature of the impediment, and to the force of the current. They obtained similar results on experimenting on the arteries of living animals; and discovered, that in the human subject, the same sounds may be produced by simple pressure, not only in the arteries, but in the veins also. They found that the sounds, heard in the neck, described by some eminent French writers, under the names, "*bruit de diable*," and "*bruit de mouche*," as signs of a particular morbid condition, which requires the use of certain remedies, may be produced at will, by the pressure of the stethoscope on the jugular veins of the healthiest persons, and is therefore not necessarily a sign of disease, but has probably been accidentally caused by the same pressure in many cases, in which it has been considered as a morbid sign.

The Committee conclude, in answer to the first inquiry, that a certain resistance to a moving current is the essential physical cause of all the various sounds in question, and that this resistance is generally given by some pressure on, or impediment in, the tube through which the current moves; but, that sometimes the resistance is caused by a change in the direction of the current, by which it is made to impinge on the walls of the vessel which contains it.

The second inquiry, the Committee think, can be fully answered only by extensive clinical and pathological observation, with due regard to the previous investigations; but they have planned some experiments, that promise to elucidate certain obscure points of the

pathology and diagnosis of diseases of the heart and arteries, the knowledge of which would be of direct practical advantage. These points the Committee propose to investigate, if the Association think fit to re-appoint them to this office.

Mr. Brett then read a paper "On the Physical and Chemical Characters of Expectoration in different Diseases of the Lungs, with some Preliminary Remarks on the Albuminous Principles existing in the Blood."

The remarks on the blood referred more particularly to a general view of the albuminous principles existing in that fluid. The simplest view which could be taken of the vital fluid, is that which refers its constitution to a mixture of fluid, or soluble and insoluble albumen—the one constituting what is termed the serum, the other the crassamentum, or cruor. The author of the paper then proceeded to relate the different opinions which had been published on the specific gravity of the blood, quoting the statements of Berzelius, Gmelin, Dumas, and other chemical philosophers of distinction; at the same time remarking, that all these statements did not differ materially from each other, and might be considered as depending upon the fact, that the specific gravity of the blood might differ slightly, not only in different individuals, but in the same individual at different times. He then noticed the different modifications of albumen existing in the serum, which he divided into three forms:—1st, Soluble or free albumen, capable of undergoing coagulation by heat; 2ndly, Albumen in combination with a basic body, viz., soda; and, 3rdly, A form of albuminous matter, which he termed the colourless self-coagulating albuminous principle.

The crassamentum, as it is commonly called, of the blood, he also considered as made up of more than one form of *solid albuminous* matter; viz., of solid albumen capable of undergoing decoloration by ablution with water, and of solid albumen incapable of being decolorated by the same process; the former being insoluble, and constituting what is commonly understood under the name of fibrine, the latter soluble in water, and frequently designated red particles or *hæmotosine*. Some remarks then followed on the microscopic examination of the blood, and on the different forms of the globules in different animals. The author then proceeded to detail the various physical characters of the expectoration in the healthy condition of the lungs, as well as in its varied morbid states. The physical characters of saliva were entered upon, and the globularity of its opaque portions alluded to. The physical characters of expectoration in the pituitous catarrh of Laennec were then detailed; also those of the expectoration in acute and chronic bronchitis—in hæmoptysis, or pulmonary apoplexy—in pneumonia—and, lastly, in different stages of phthisis. The chemical characters of these different modifications of expectoration were then fully treated of, and reference made to a tabular arrangement which the author had embodied in his papers, exhibiting the action of certain re-agents—first, on saliva, and then on the differ-

ent forms of sputa, the physical characters of which had been already fully noticed. It was remarked that saliva did not contain any soluble albumen capable of undergoing coagulation by heat; neither did it contain any solid albuminous matter, the main bulk of the solid contents of that secretion being mucous. The mode of analysis adopted was, to deject saliva in cold water, and then subject the filtered fluid to the action of certain re-agents; another portion of saliva was then dissolved in a caustic alkali, and the alkaline solution subjected to the agency of certain tests. The quantity of solid matter in a given weight of saliva was also announced, as well as the saline matters, and their chemical nature stated: different authorities bearing on the subject were quoted, especially the statements of Berzelius and L. Gmelin. The chemical characters of expectoration in pituitous catarrh were then described, and a mode of analysis was stated to have been adopted analogous to that employed in the case of saliva: this modification of sputum was regarded as purely mucus, possessing no albuminous matter; it was found to contain a very small proportion of solid matter in a given weight, but the quantity of saline matter was found to be considerable, when compared with the quantity of solid matter; and this saline matter the author's experiments lead him to conclude was diminished in quantity as the disease progressed. The chemical nature of sputum of the acute and chronic bronchitic character was then entered upon, and noticed as differing in certain respects from the preceding form of expectorated matter, in containing, for example, a much larger proportion of solid matter in a given weight than was found in simple pituitous expectoration, and generally a smaller proportion of saline matter; it also differed in containing, generally speaking, small quantities of soluble albumen capable of undergoing coagulation by heat.

Pneumonic expectoration was then treated of, and noticed as principally made up of a tough, mucoid secretion, intermixed with blood, to which last was owing its peculiar rust or brick-red colour, and also its powers of undergoing, to a certain extent, coagulation by heat when mixed with water and filtered; it was also found to differ from most other forms of expectorated matter, in containing no inconsiderable quantity of oxide of iron, derivable from the blood with which it is impregnated. Phthisical expectoration was the last form of sputum, the chemical characters of which were described. It was noticed as differing materially in different stages of the disease; in the earlier and middle stages scarcely not at all; for the most part, at least, differing from the expectoration met with in chronic or acute bronchitic affections; in the latter stages, however, not unfrequently possessing the characters of a simple collection of puriform matter, containing very large quantities both of soluble and solid or insoluble albumen, much solid matter also in a given weight, with the ordinary saline matters found in other varieties of sputa, superadded to which was a notable proportion of oxide of iron. It was stated that in no disease, except phthisis, did the expectoration contain so much soluble albumen

capable of undergoing coagulation by heat ; and also in no disease except pneumonia, was there so large a proportion of solid matter in a given weight of the expectoration : this observation referring, however, to the sputum in the latter stages of phthisis, where it puts on the character of a collection of puriform matter. Allusion was then made to the fatty matter existing in expectorated fluids, which was found to be the same in quality in almost every variety of sputum, but differing in quantity, being much greater in quantity in well-marked phthisical expectoration than in any other variety. The fatter matter was peculiar, too, from the high temperature which it required for its fusion, it being considerably higher than that necessary for the fusion of the more ordinary forms of fatty matter, and even higher than that required for cholesteroline ; this fatty matter was soluble in alcohol and ether, being deposited from the former when its boiling solution cooled. The author also referred to the power which a galvanic current, even of low intensity, possessed of coagulating the aqueous fluid, obtained either by digesting saliva, or any of the modifications of expectorated matter before alluded to, in water, and filtering the fluid. This coagulation was not regarded by the author as proving the presence of albumen, because, in cases in which the galvanic current effected the change in question, the most delicate re-agents with which chemists are acquainted for the detection of albumen, failed to detect the slightest trace. The author then detailed his experiments on crude and softened tuberculous matter ; he submitted the former to the action of the same re-agents as he employed to re-act upon ordinary fibrine, and was led to conclude that the crude tubercle did not differ chemically from solid albumen or fibrinous matters. The mode of analysis employed in examining the crude and softened tubercle, was the following : it may be observed, that the crude tubercle was examined side by side, with ordinary fibrine ; the crude tubercle was dissolved in a weak solution of caustic potass ; a similar solution of fibrinous matter was obtained, and both submitted to the action of the same re-agents, with results as nearly similar as possible. The agents employed were the mineral acids, acetic acid, and ferrocyanide of potassium, tinct. galls, corrosive sublimate, &c. The softened tuberculous matter was first dejected in water, and then filtered ; the filtered fluid, when submitted to re-agents, was found to contain soluble albumen ; that portion of the tubercle insoluble in water, was dejected in a weak alkaline fluid, by which a solution was obtained. This alkaline solution, when submitted to the necessary re-agents, indicated the existence of solid albuminous matter or fibrine ; hence, the softened tubercle was regarded as analogous in its chemical characters to purulent matter. Experiments were then made on the tuberculous matter which had undergone perfect softening, and the result was, that the latter was chemically identical with pus ; from which it was deduced, that fibrinous matter was, by a process of softening or fluidfaction, converted into actual pus ; and hence, a fruitful source of the abundantly albuminous fluid found in the ex-

pectoration of patients, in the latter stage of phthisical disease. The author then concluded his paper, by stating the results of a quantitative analysis of the expectoration of a marked puriform character, obtained from a patient in the last stage of phthisis. It was found to consist of—water; albuminous matter, with a little mucus; extractives, soluble in alcohol; ditto, soluble in water; fatty matter; saline matters, consisting of the alkaline chlorides, phosphates and carbonates, with earthy phosphates and oxide of iron.

Dr. John Reid then gave an account of an experimental investigation into the Glosso-pharyngeal, Pneumogastric, and Spinal accessory Nerves. This communication was stated by Dr. Reid to contain merely a short epitome of some lengthened remarks, which he had drawn up on this subject; but it embraced the principal results, deduced from the numerous experiments which he had performed upon those complicated and important nerves, generally included under the eighth pair.

Glosso-pharyngeal Nerve.—The experiments on this nerve were all performed on dogs, and were twenty-seven in number. Seventeen of these were for the purpose of ascertaining if it was to be considered a nerve both of sensation and motion, and what were the effects of its section upon the associated movements of deglutition, and on the sense of taste. The other ten were performed on animals immediately after they had been deprived of sensation, with the view of ascertaining more accurately how far it is to be considered a motor nerve. The phenomena observed in these experiments were first stated; and the conclusions drawn from a review of the whole of the data thus obtained were these: first, that this is a nerve of common sensation; second, that mechanical and chemical irritation of this nerve, before it has given off its pharyngeal branches, or of any of those branches individually, is followed by extensive muscular movements of the throat and lower part of the face; third, that the muscular movements thus excited, depend, not upon any influence extending along the branches of the nerve to the muscles moved, but upon a reflex action, transmitted through the central organs of the nervous system; fourth, that these pharyngeal branches of the glosso-pharyngeal nerve possess endowments connected with the peculiar sensations of the mucous membrane, upon which they are distributed, though he cannot pretend to say positively in what these consist; fifth, that this cannot be the sole nerve upon which all these sensations depend, since the perfect division of the trunk on both sides, with removal of a considerable part of it (if care be taken to exclude the pharyngeal branch of the par vagum, which lies in close contact with it), does not interfere with the perfect performance of the function of deglutition; sixth, that mechanical or chemical irritation of the nerve, immediately after an animal has been killed, is not followed by any muscular movements, provided care be taken to insulate it from the pharyngeal branch of the par vagum; seventh, that the sense of taste is sufficiently acute after the perfect

section of the nerve on both sides, to enable the animal readily to recognize bitter substances; eighth, that it may probably participate with other nerves, in the performance of the function of the sense of taste, but it certainly is not the special nerve of that sense; ninth, that the sense of thirst does not depend entirely upon this nerve.

Pneumogastric, or Par Vagus Nerve.—From the result of thirty experiments on this nerve, he is satisfied that severe indications of suffering are generally induced by pinching, cutting, or even stretching this nerve. Powerful respiratory movements were excited in some of the animals, when the trunk of the nerve was compressed for a few moments by the forceps.

Pharyngeal Branches of the Par Vagus.—From seventeen experiments performed on dogs, either when alive, or immediately after being deprived of sensation, he concludes, that these are the sole motor nerves of the constrictors of the pharynx, the stylo-pharyngeal and palatine muscles; and that the sensitive filaments contained in these branches of the par vagum are exceeding few, if under ordinary circumstances they exist at all.

Pharyngeal Branches of the Par Vagus.—From his experiments on these nerves, repeated and confirmed in various ways, he concludes, that all the muscles which move the arytenoid cartilages, receive their motor filaments from the inferior laryngeal or recurrent nerves. That one only of the intrinsic muscles of the larynx receives its motor filaments from the superior laryngeal, viz. the crico-thyroid muscle, and that consequently, the only change which this nerve can produce upon the larynx as a motor nerve is, that of approximating the cricoid cartilage to the thyroid,—in other words, of shortening the larynx; and that the sensations of the larynx depend upon the superior laryngeal nerve. These experiments are completely subversive of the statement of Majendie, that the inferior laryngeal nerves supply those muscles only which open the glottis, while the superior supply muscles which shorten the glottis. They also illustrate, in a very satisfactory manner, the causes of the dyspnœa, amounting in some cases to strangulation, when the inferior laryngeal nerves are cut or compressed.

He has also satisfied himself, that when any irritation is applied to the mucous membrane of the larynx in the natural state, this does not excite the contractions of these muscles, by acting directly upon them through the mucous membrane, but that this contraction takes place by a reflex action, in the performance of which the superior laryngeal nerve is the sensitive, and the inferior laryngeal is the motor nerve. He is also convinced that the muscular contractions of the œsophagus are not called into action by the ingesta, acting directly as an excitant upon the muscular fibre, through the mucous membrane, but by a reflex action, part of the œsophageal filaments acting as sensitive, and others as motor nerves. Our space will not permit us to state any of the results obtained from the experiments on the other branches of the par vagum.

Spinal Accessory.—This nerve was cut across in seven dogs at

its exit from the cranium, and no effect upon the voluntary movements of the muscles of the neck could be observed. In other animals the nerve was first cut across on one side, and then a weak dose of prussic acid given. This was frequently followed by powerful, slow, and regular respiratory movements, during some of which distinct contractions of the sterno-mastoid muscle were observed in unison with the other muscles of inspiration.—*Medico-Chirurgical Review*.

TO THE EDITORS OF THE DUBLIN MEDICAL JOURNAL.

Harcourt-street, October, 10th, 1837.

GENTLEMEN,

I must beg of you to correct an error occurring in the notes taken of a paper, read by me on Transplantation of the Cornea, before the College of Physicians, and published in the Dublin Medical Journal. In them it stated, that previous to operation, I secured the upper eyelid by passing a ligature through it. This is not the case: what I expressed to the meeting was, that "I passed (in operating on animals), a needle armed with a fine ligature, through the third eyelid, and secured it to the upper eyelid to prevent its protrusion," as that was the only means which I found capable of keeping it out of the way. As portions of those notes have been re-published in other periodicals, to prevent the mistake going farther, you will oblige by inserting this correction.

Ever yours truly,

S. L. L. BIGGER.

Death of Dr. William West.—We do not know when a more painful duty has devolved upon us than that of recording the death of our amiable and distinguished fellow-citizen, Dr. William West, son of Alderman Jacob West, of this city. When we consider the estimation in which this individual was held as a sound and erudite scholar by the most learned men of the day; the ardent zeal with which he prosecuted science, and the premature age (only thirty-four) at which he was cut off, it is not too much to say, that his loss was a public one, and one that must be deeply deplored by the world of letters. The promises held out by his collegiate success were more than realized in the distinctions his labours in medicine, philology, geography, and botany, subsequently acquired for him. Of these subjects, the science of languages occupied his especial attention; and so accurate and profound was his knowledge in this branch of literature, that in matters of abstruse research, his assistance was urgently sought after by the most eminent philologists both at home and abroad. His capabilities in this respect were practically and disinterestedly applied to, rendering the most recent discoveries in medicine throughout Europe available to his professional brethren at home. The purity of mind, singleness of purpose, and almost childish simplicity of thought, in the ordinary affairs of the world, so often the characteristic accompaniments of true genius, were leading features in Dr. West's character. He

may be said to have fallen a sacrifice to his too ardent pursuit of science, as the fatal fever, which terminated his short but distinguished career, was caused by close application, whilst in a delicate state of health, in preparing for the British Association an elaborate critical illustration, through its primitive dialects, of the Ancient Geography and History of Gaul and the British Isles.—*Evening Paper.*

Sir A. Cooper in Edinburgh.—On Tuesday, September 5th, the College of Surgeons of Edinburgh entertained Sir Astley Cooper to dinner in the Royal Hotel. Besides a numerous attendance of the College, there were present Dr. Abercrombie, Professors Hope, Alison, Sir C. Bell, &c., and many medical gentlemen formerly in the public service. Sir G. Ballingall was in the chair; and Mr. Lothian, Treasurer, Drs. Gardiner and J. Campbell, acted as croupiers. After the usual loyal toasts, the health of Sir Astley Cooper was drunk with the warmest enthusiasm, which Sir Astley acknowledged in a speech of great good humour, and the utmost kindness of feeling. He stated, that fifty years ago, in the year 1787, considering that his medical education could not be complete without studying in Edinburgh, he became the pupil of Cullen, Black, Gregory, &c. He mentioned, that when he came to Edinburgh, on Wednesday, the first place to which he drove was his former lodgings in Bristo-street; that amidst all the improvements in Edinburgh, he found the locality of his former residence very little changed; that he saw the very room and the closet, endeared by former recollections; and that when he looked back upon those days, and reflected on the fame and success which had attended him through life, he was tempted to fall down on his knees and thank that Providence which had so conducted him. Seeing so many members of the profession in the company, he affectionately exhorted them to attain the utmost skill in their profession, never to attempt to rise by depressing another, and never to let go their integrity and high moral character. He paid high compliments to the great men under whom he had studied, and was delighted to find that the reputation of the University was still upheld by those who had succeeded him. He highly approved of the adoption of Sir C. Bell as professor of surgery. In the course of the evening, Sir G. Ballingall announced that he had been informed by letter from the Lord Provost, who regretted that his health would not allow him to be present at the meeting, as he had been invited to be, that the freedom of the city of Edinburgh had that day been conferred upon Sir Astley, whose health he again proposed in the capacity of the youngest citizen of Edinburgh. The evening was spent throughout in the utmost harmony, and the meeting did not separate till a late hour.—*Edin. Paper.*

THE
DUBLIN JOURNAL
OF
MEDICAL SCIENCE,

1 JANUARY, 1838.

PART I.
ORIGINAL COMMUNICATIONS.

ART. XVIII.—*Case of Rupture of an Aneurism of the Common Carotid, and Ligature of that Artery near its Origin from the Innominata.* By T. ARGYLL ROBERTSON, M. D., F. R. S. E., Lecturer on Surgery in Edinburgh.

THE following instance of aneurism, besides being interesting in several points, appears more particularly worthy of being placed upon record, as being, up to the present time, as far as I am aware, the case in which a ligature has been applied to one of the larger arteries of the body, *nearest* to their common origin from the heart, with *perfect ultimate* success.

Major ———, the subject of the case, is now in his fifty-second year. In April, 1836, while hunting, his horse, when at full speed, put its foot into a rabbit hole, by which both it and its rider were brought to the ground with great violence. Major ——— received a severe wound over the left parietal bone, which bled profusely, and he remained for a short time stunned, and in a state of insensibility. From that

period he suffered from stiffness and pain in the right side of the neck, resembling what is usually termed a crick, accompanied by shooting pains over the whole right side of the head : occasional attacks of giddiness, slight strabismus, and double vision followed, the last to so great a degree that he was forced to give up his favourite amusement of hunting, every gate, fence, &c. appearing to him double. The sight was particularly confused when looking straight forward, and still more so to the right. He likewise lost all idea of distances. On shutting either eye vision again became distinct. The motions of the iris in each eye were perfectly natural. The appetite was good, and bowels regular. Pulse rather full, but in other respects natural. In addition to the above symptoms, in December, 1836, he began to suffer occasionally from a feeling of numbness and coldness of the left arm and leg. At this period he placed himself under the care of Professor Alison and myself. Looking upon the symptoms as indicating a tendency to an apoplectic attack, we placed him upon a regulated diet, his bowels were kept open, leeches were ordered every second or third day to the temples or behind the ears ; he had cupping glasses applied upon two or three occasions. The head was shaved, and sponged several times a day with cold water, and he was desired to take regular but moderate exercise, and to reside quietly at his seat in the country. Under this treatment his health was much improved, and his vision almost perfectly restored.

About the middle of January last he first discovered a swelling on the right side of the neck, accompanied by enlarged tonsils, slight sore throat, and some difficulty in swallowing. The swelling in the neck was supposed to be simply an enlarged gland, and did not attract particular notice. Leeches were ordered to be applied from time to time, more especially if it should inflame or become painful.

On the 20th of March, without any premonitory symptoms, at 10 o'clock, P. M., a sudden gush of blood took place from the mouth ; it was discharged in gulps or mouthfuls in rapid suc-

cession ; it ceased spontaneously after half an ordinary wash-hand basinful of blood had been lost ; he retired to bed, and slept very soundly during the whole night.

The following morning he rose at 8 o'clock, but had scarcely reached his dressing room when the hæmorrhage returned, and, to use his own expression, the blood literally poured from his mouth ; he soon fainted and fell upon the floor, breaking a foot pail in the fall ; about fifty ounces of blood were at this time discharged by the mouth, and a considerable quantity must have passed into the stomach, as the stools afterwards consisted almost entirely of coagulated and grumous blood. By the two hæmorrhages he must have lost upwards of one hundred ounces of blood. The bleeding now ceased, and he rallied a little and procured assistance. He remembers distinctly that at this time he shook very much, and felt excessively cold. Having taken a laxative the previous night, he had now an urgent call to stool, and was carried by his servant to the water closet, when he had a copious alvine evacuation, after which he again fainted. On recovering he still shook very much, and felt very cold ; he was put to bed, and warm bottles were applied to the feet. Dr. M^cIntyre of Muthill, having been sent for, arrived about 10 o'clock, and found Major ——— in a very exhausted state, his extremities cold, the pulse at the wrist scarcely perceptible, and giving forty-five beats in a minute. Dr. M^cIntyre, perceiving the urgent nature of the case, sent off an express to Edinburgh for me. I reached Major ———'s seat about midnight, and found him perfectly composed and tranquil, with the pulse scarcely perceptible at the wrist, and continuing to beat about forty-five strokes in the minute. On examining the neck I found a tumour extending from near the angle of the jaw to *within one inch* of the sternum, and projecting laterally to about three inches. Its surface was smooth, equal, and rounded, and a very obscure pulsation could be detected. Judging from the state of the circulation, and from there not having been the slightest return of

bleeding since 8, A. M. that there was not immediate danger, I thought proper to postpone attempting to secure the carotid below the seat of the aneurismal swelling, (evidently the only surgical resource that was left me), in order that I might have the benefit of day-light for the operation; and I sent off to Stirling for Mr. Forrest, that we might have the advantage of his advice and assistance in so important a case.

In consequence of the aneurism being seated so low down in the neck, the external incision was limited to little more than an inch, following the course of the sterno-mastoid from the sternum upwards. On dividing transversely the sterno-thyroid and a few fibres of the sterno-hyoid muscles, a narrow projection of the aneurismal tumour, passing between the artery and trachea, was brought into view; so narrow that I at first supposed it to be the artery somewhat dilated, and passed the aneurism needle round it. It was, however, about double the size of the artery, and its coats were thinner than natural. On examining the parts more minutely, I discovered the carotid displaced laterally by this prolongation of the sac. The pulsations were feeble, though perfectly distinct; the situation at which it was exposed was within a finger's breadth of its origin from the innominate, and when the finger was applied to this point the carotid was felt pulsating on its palmar,—the innominate on its lateral surface. Immediately above this point the vessel swelled out into the aneurismal tumour. The ligature was therefore applied *within half an inch* of the *origin* of the artery. During the performance of the operation neither vein nor nerves were seen. The operation itself was necessarily tedious and difficult, in consequence of the limited extent of the external incision, the deep situation and unnatural displacement of the artery, and the importance of the organs by which it is surrounded. At one time I thought it would have been necessary to have divided the sternal attachment of the sterno-mastoid, but this was avoided by relaxing the muscle and drawing it outwards. The vessel was no further separated from its

attachments than was necessary for the simple passage of the aneurism needle. On tightening the ligature all pulsation ceased in the tumour, and it was reduced nearly a third in bulk. No peculiar sensations were experienced by the patient, who bore the operation with the greatest possible fortitude. He was placed in bed, with the head considerably elevated to relax the parts, the lips of the wound having been previously brought together by suture, and supported by a strip of adhesive plaster. Strict antiphlogistic regimen and perfect rest were enjoined. The bowels were regulated by enemata to avoid any risk of sickness, vomiting, or hypercatharsis from the exhibition of cathartics. The wound healed by granulation; at first the discharge was thin and slightly tinged with blood, but gradually it assumed the characters of healthy pus.

On the seventeenth day the ligature separated, and the wound speedily healed. The aneurismal tumour rapidly disappeared, and now no trace of it whatever can be discovered. From the period at which the ligature was applied up to the completion of the cure not an untoward symptom appeared. On the second day after the operation the pulsation in the branches of the external carotid was distinct. During the third week a few drops of blood were discharged from the right nostril, accompanied by a little irritation giving rise to a great desire to sneeze, probably depending on the new arrangement of the circulation.

At the present time, 28th September, six months from the date of the operation, Major —— is in the enjoyment of the most perfect health.

The preceding case of carotid aneurism may be regarded as one of very considerable interest in various pathological and surgical points of view. The aneurismal state of the affected carotid (the right) appears to have originated in a sudden twist of the neck, occasioned by a blow on the opposite or left temple. The disease occurred in the trunk of the common carotid at some distance below its division, a situation in which aneurism is very rarely seen. The more prominent symptoms were those

indicating disease of the brain, and probably arose from an altered state of the circulation through that organ, as they have completely disappeared since the aneurism was cured. The local symptoms of the aneurism were so very trifling as not to attract particular attention, proving the necessity of a minute examination of all swellings in the vicinity of large arteries. The various instances related by Vesalius, De Haen, Palfin Harderus, Warner, Scarpa, Richerand, Dupuytren, &c., in which aneurism of the carotid has been mistaken, even by well-informed surgeons, for a suppurating gland, and laid open so freely as to give rise to instant and fatal hæmorrhage, illustrate the same point, and afford us striking lessons for caution in all cases that are in the least degree doubtful.

The difficulty experienced in performing the operation, from the bulk of the tumour limiting the extent of the incision, and rendering it necessary to apply the ligature nearer to the heart than might otherwise have been required, where the vessel is so deeply seated and surrounded by important parts, and still more the imminent risk of immediate death to which the patient was exposed from the rupture of the aneurism, point out the danger that would arise from delaying the operation. It is probable that the previous depleting and antiphlogistic treatment, and the large quantity of blood lost (about 100 ounces) immediately before the operation, mainly contributed to the successful result in this particular instance, an opinion which may appear in some degree borne out by the fact, that there is not, as I have already remarked, any previous case on record, as far as I know, in which the application of a ligature to an artery so near the heart has not been followed by a fatal result.

ART. XIX.—*On the Frequency of the Presence of the Trichocephalus Dispar in the Human Intestines.* By O'B. Bellingham, M. D., Second Surgeon to St. Vincent's Hospital, &c. &c.

[Read at the Natural History Section of the British Association at the Liverpool Meeting, in September, 1837.]

No point in the whole range of pathology is surrounded with greater difficulties or involved in greater obscurity than the origin of those parasitic animals whose natural and permanent habitat is the interior of the body of man, and other animals; we have a repugnance to allow to animals the power of creating, or (if I may use the expression) of secreting animal life; yet, in what other way can we account for the origin of the spermatozoæ of the human semen, and of the semen of those animals in which they are only periodically found, unless we allow them to be the product of secretion? But in fact of secretion, and the manner in which it is effected, we know next to nothing; we learn little from an examination of the organs of secretion: no person could tell, *à priori*, that the liver must secrete bile; or the kidneys, urine; or the testicles, semen; we merely know the facts, but of the *quo modo* by which the process is effected, we are as much in the dark, as we are of that by which the entozoæ are first called into existence in an animal. However, experience has taught us that certain causes tend to the production of these parasites; we can, by keeping an animal in an unnatural condition, cause the generation of certain species, and we know, that once produced, they have the power of propagating and continuing the species even to a degree inconsistent with the health of the animal which nourishes them. Passing over then the question as to how they are originally or primarily generated, as one which our present limited modes of investigation will probably never allow us to decide, it may be more useful to restrict our researches to determine, whether the mere presence of these animals in the intestines must of necessity be injurious,

and whether they may not exist in considerable numbers even in the human subject without causing the slightest inconvenience.

Many diseases were formerly supposed either to have their origin in, or to be aggravated by the presence of worms in the intestinal canal; but it is well known that in almost every animal we examine, one or more species of these parasites is found, although the individual, to all appearance, enjoyed perfect health: from analogy, we might suppose the same to be the case with the human subject, which I think can be satisfactorily proved.

It is not uncommon to find the *tænia solium* in the intestinal canal of individuals who never complained during their lives of any symptom of tape worm; I can say the same of the *oxyuris vermicularis*, and the *trichocephalus dispar*, I have found in the large intestines of almost every individual whom I have examined.

In the observations I am about to make, I shall confine myself to the last mentioned species, as among the entozoa, peculiar to the human species, it has more frequently been described incorrectly, and it appears to be less known in this country than any other, although by many degrees the most common. In works published in England, the authors of which treat, either incidentally or otherwise, of the human entozoa, very erroneous statements are put forward respecting it, and my object (in this short paper) is, not to give a complete history or anatomical description of this animal, but merely to point out some of these inaccuracies, and to describe what I have myself observed.

The genus *Trichocephalus* (derived from $\theta\rho\iota\xi$, a hair, and $\kappa\epsilon\phi\alpha\lambda\eta$, head,) is characterized by a cylindrical and elastic body, the anterior part being capillary, and passing abruptly into the thicker, by which it is distinguished from the genus *Trichosoma*, in which the increase is very gradual; the mouth is orbicular, the male organ a simple spiculum contained in a sheath. It belongs to the order *Nematoidea* of Rudolphi and Bremser, and

to the division "*Vers Intestinaux Cavitaires*," in Cuvier's *Regne Animâle*.

The genus *Trichocephalus* is not numerous in species, only eight being described by Rudolphi. This particular species (the *Trichocephalus Dispar*) was formerly called *trichuris* (derived from $\theta\rho\iota\xi$, a hair, and $\sigma\upsilon\rho\alpha$, a tail,) from the erroneous supposition that the capillary portion was the tail of the animal; it is from an inch and a half to two inches long, the capillary part composing about two-thirds of its length, at the extremity of which is its mouth. The male is smaller than the female, and the thicker part of its body is spirally twisted, in the female it is straight or nearly so; the penis of the male is a simple spiculum, contained in a sheath, the orifice of which is somewhat bell-shaped. The opening, which, in the female, serves the purpose both of vagina and anus, is small, and situated close to the termination of the posterior extremity.

The part of the intestinal canal which the *trichocephalus dispar* most commonly occupies is the cæcum, more particularly the neighbourhood of the ilio-cæcal valve; I have found them however through the whole tract of the colon, in the ileum close to the cæcum, and in the appendix vermiformis. We sometimes find the head of the animal implanted in the mucous membrane, lining the intestine; but this is rare; more commonly they are quite unattached, and when an opening is made into the cæcum, they come out in its fluid contents; although one of the latest writers on this subject says, "the head of the animal is commonly found implanted in the walls of the intestines, while the other end moves freely through the fæces."

The *trichocephalus dispar* has not been so long known as the other parasites of the human intestines; its discovery was made accidentally, in the winter of 1760—61, at the Anatomical Theatre of Göttingen; its first discoverers indeed mistook the male and female for distinct species. About this time, an epidemic (called by the writers of that period *morbus mucosus*)

raged in a division of the French army stationed at Göttingen, and as these worms were very frequently found in the bodies of the soldiers who died of it, the conclusion come to was, that they either were a cause or the effect of the disease ; on which (as it will appear that they are found in almost every individual) they could have had no influence. Recently a somewhat similar occurrence has taken place at Naples, the physicians to the Hospital d'ella Conzolazione in that city have been examining the intestines of all those who died of cholera, and constantly meeting with these worms in the large intestines, the discovery appears to have delighted them not a little, and they almost venture the opinion that a probable cause of this disease has at length been discovered.*

I have already observed that the discoverers of this animal mistook its tail for its head, and named it *trichuris* ; however, after greater care had been given to its examination, the error

“ ‘ The only points which seem peculiar,’ (says M. Thibault, in his account of the *post mortem* appearances of those who died of cholera at Naples, communicated to the Royal Academy of Medicine,) ‘ and worthy of notice, are the presence of a rare species of worm, and of a vast quantity of thick and viscid mucus in the intestinal canal.’ ”

“ The discovery of this fact by the young Professor Ramoglia, was at first thought to explain the existence of cholera, for it had been ascertained, beyond doubt, that the appearance of this worm (which is a very rare one) did not precede that of cholera. On maturer reflection, however, the Neapolitan physicians determined to extend their inquiries to persons not dying of cholera, and by this sage precaution they have thrown very considerable light upon the nature of that disease. Thus, for example : the presence of the *trichocephalus* worm was determined in eighty non-choleric bodies, (without exception,) examined between the 10th of December and the 1st of February. They were generally in less quantity than in the bodies of choleric patients, but occupied the same places, and were exactly similar.

The results deduced by M. Thibault from the observations of the Neapolitan physicians, were :

“ 1st. ‘ That the cholera at Naples was complicated with one of the most extensive verminous epidemics known in history.

“ 2nd. ‘ That this epidemic was not confined to choleric patients, but affected all the inhabitants of the place.’ ” — *LANCET*, Oct. 28th, 1837.

was corrected, and it was named trichocephalus ; it is then not a little extraordinary that in works published in England within the last few years, the original error should have been repeated. In Baillie's works, edited by Wardrop, and published in the year 1825, we find the following ; " It (the trichocephalus dispar) has a very long transparent tail, to their heads is attached a process or horn, which they can protrude or retract." This process or horn being the penis of the male and its sheath.

Monroe in his *Morbid Anatomy of the Gullet, Stomach and Intestines*, 2nd edition, published at Edinburgh in 1830, says : " it has a long filiform tail ;" " different authors," he afterwards adds, " vary in their opinion respecting the anatomy of this worm ; according to some it has a proboscis, which it can eject at pleasure, according to Göetze that is the penis of the animal."

With respect to the frequency of the presence of the trichocephalus dispar in the human intestines, the same authors, as well as later* writers in this country, are no less in error. Dr. Baillie, in his large work on *Morbid Anatomy*, says : " this worm (the trichuris) is not only rare in this country, but I believe in every other." In the edition of his works already quoted, he says : " the trichuris has occasionally been found in the great intestines of man, and more especially in the cæcum." Monroe (in his work before alluded to,) says : " this species is rarer than any of the other intestinal worms." Dr. Hodgkin in his admirable *Lectures on Mucous and Serous Membranes*, says : " on the Continent it would seem that this is the worm most frequently met with in the human intestines, indeed some of the most distinguished helminthologists state that they scarcely ever fail to find them. Although I have frequently and care-

* I do not include the authors of the excellent articles in the *Cyclopædias of Anatomy and Physiology*, and of *Medicine*, as the writers do not give their own experience as to their frequency or otherwise, but quote foreign authors on this point.

fully sought for it, I have only once been able to find it; in this instance it was lodged in the mucus filling the appendix of an emaciated and cachectic girl."

On the other hand, most French and German writers (who speak of them) state that they are very common. Both Rudolphi and Bremser agree on this point. Cruveilhier says: "in every epidemic of acute follicular enteritis, I have met with a greater or less number of them, and they are very often found in the bodies of those females who die of puerperal peritonitis at La Maternite; Bremser," (he continues,) "says that they exist in almost every male subject, but I have in vain sought for them in a great number of cases, and rarely are they found in great numbers, though Rudolphi states that he has seen more than a thousand in the same individual. M. Gentrac of Bourdeaux, in an article in the *Revue Medicale*, says, "the cæcum of most individuals, healthy or sick, young or old, contains almost constantly trichocephali; it is long since I made this observation, I have repeated and given it publicity in my courses of anatomy during the last fifteen years at the School of Medicine in this city. Every time," (he continues,) "that I have been describing the large intestines, I have remarked as a circumstance worthy of attention, that these organs, and particularly the cæcum, are constantly the receptacle, the habitual domicile of these perfectly innocent worms. Their presence has been verified in persons dying suddenly in good health, as in criminals; I must add however," (he continues,) "that wishing to determine if, in the first periods of life, similar worms were found, I have searched with care for them in the intestines of numerous newly born infants, but without success."

With a view to determine between the contradictory statements of the English and continental pathologists, I examined successively the intestinal canal of twenty-nine individuals who died in St. Vincent's Hospital during the last twelve months, (of whom eleven were males and eighteen females,) and in twenty-six out of the twenty-nine I found a greater or less

number of these worms. The ages of these individuals varied much, the youngest being but eight years old, and the oldest upwards of seventy. The diseases which proved fatal to them also varied, some died of injuries, others of acute, and others of chronic diseases. In several instances I found but two or three trichocephali; in others upwards of eighty; the largest number having been found in a boy aged 14, who died of dropsy, with disease of heart and kidneys, in whom I counted one hundred and nineteen. In some instances I examined the proportion which the male trichocephali bore to the females; in one individual I found nineteen males and twenty-five females; in another sixty-one males and twenty-four females; in another one male and one female; in another four, all males; and in another six, all females. The three individuals in whom I failed in detecting these worms, were females; one died of scirrhus of the pylorus, and the communication between the stomach and duodenum, was so contracted, as not to admit even a probe to pass, hence we may naturally suppose, that if they had previously existed they were starved out; another died of cancer, which commenced close to the orifice of the urethra; and the third died of protracted diarrhœa, with extensive ulceration of the mucous membrane of the cæcum, colon, and lower part of the ileum; she had been taking for some time before her death a combination of sulphas cupri with opium and other medicines, which most likely acted as poisons to these animals, and caused their expulsion.

Not one of the twenty-six individuals in whom these worms existed had complained, either before or during the illness which proved fatal, of any symptom which could lead to the suspicion that these worms had been in the least degree prejudicial to their health.

ART. XX.—*Contributions to the Pathology of the Uterus.*

By JOHN BROWNE, M. D., one of the Medical Attendants of St. Mark's Hospital and Dispensary, &c.

[Read before the Surgical Society of Ireland.]

FOUR specimens of different uteric lesions are here laid before the Society, and as I possess a history of every case, which in itself presented circumstances more or less interesting, I am in hopes that the entire may not prove unworthy of the attention of this meeting.

I shall first explain each preparation, next give a detail of the symptoms and treatment, and lastly make a few observations on such facts as appear to me worthy of notice.

The first is a fine example of laceration of the vagina and uterus during parturition. The vagina is observed to have been extensively torn in front and laterally, almost all round, and nearly in a transverse direction; pretty much about the place of its junction with the uterus. The peritoneum is seen to have been torn also in front and laterally, for about six inches in length, and on the left side the muscular tissue of the uterus has been implicated for an extent of three inches, but here the uterine cavity was not exposed, one-half of the thickness of the organ only having suffered.

The torn peritoneum and uterus seem to have formed a sort of flap directed somewhat towards the left side, while the laceration of the vagina inclined more downwards and towards the right; the edges of the wound are observed to be separated about four inches.

The other necroscopic phenomena noticed at the *post mortem* examination, (which took place but two hours after death,) were these: two pints of bloody fluid within the abdominal cavity; the omentum gathered up into a mass and lying to the right side, the lower half of this membrane, the entire of the jejunum and ileon, the cæcum and appendix, and about six

inches of the commencement of the ascending colon, of a dark mahogany colour, as if gangrenous ; slight marks of inflammation on the abdominal peritoneum and peritoneal covering of the uterus. The last named organ, contracted to about its usual size at the fourth month, lay superficial at the lower part of the abdomen, rather to the right side, having the omentum in contact with it ; on removing which a large, bloody coagulum, covering the rupture, and in contact with the peritoneum, was discovered.

The particulars of the case were these : she was woman of middle size, sanguine temperament, fair complexion, aged thirty, and this was her fourth pregnancy.* Twelve hours after the commencement of labour she was seen by a midwife, who stated the pains to have been strong, the head and funis presenting. In twelve hours more, the membranes ruptured, with a large discharge of liquor amnii, recession of the presenting parts, sudden cessation of pains, and the ordinary phenomena of a ruptured uterus. The next morning (thirty-six hours from the commencement of labour) she complained of abdominal soreness near the pubes, with vomiting and slight hæmorrhage ; for which symptoms she was bled from the arm to ℥xviij. and had an enema administered.

It was not until the fourth day of her illness, or about ninety-four hours from the commencement of her labour, that she came under my care ; she then had some of the symptoms of strangulated hernia, such as stercoraceous vomiting and hiccup, besides a general soreness and flaccidity of the abdomen ; some portions of the foetus were felt superficially through the abdominal coverings. The laceration was detected by the finger, on the left side of the vagina, extending upwards about two inches, and no part of the foetus could be felt presenting. The pulse was 160, soft and very weak, and the respiration forty-four.

* I am unable to give any particulars of her previous labours.

Hopeless as the case was, I considered it to be my duty to perform the operation of turning, which was accordingly effected in the usual way, and well sustained by the patient, who, although the pulse ceased, survived about four hours.

The distance between the pubes and sacral promontory was less than usual, so that the passage of the hand was impeded. The cavity of the uterus was large, relaxed, and filled with coagula, in the midst of which was found the placenta detached. A discharge of half a pint of venous blood followed the extraction, and several folds of intestine were felt towards the fundus, both before and after the operation.

The foetus had been dead several hours, and was much decomposed, having been probably deprived of life, soon after labour commenced, by pressure upon the funis. It appeared to have presented naturally with the vertex, but the left lower extremity at this time had passed through the rent; among the intestines, there was no perceptible uterine action.

This case adds but another to the many already on record of one of the most lamentable casualties to which the human female is liable during the whole period of her parturition. The deplorable ignorance of the attending midwife is much to be regretted.* Had she called in medical assistance at first,

* I have often thought that much mischief might be prevented by an improved system in the education of our midwives, similar to that adopted in France and Germany. They are, in those countries, of a better class in society, taught their profession more extensively and carefully, and commence their studies at a period (fifteen or sixteen years) when the mind is ripe for learning and susceptible of retaining it. I shall never forget the surprise and gratification which I experienced whilst going through the obstetric wards at Vienna, on being addressed in French by a modest and elegant young female, (one of the midwifery pupils,) who politely explained the economy of the department; my imperfect knowledge of the German language preventing me from comprehending Professor Klein in his vernacular tongue. Could our national customs permit us to substitute intelligent and educated young ladies for stupid and ignorant old nurses, a great national benefit, I have no doubt, would be achieved. Dr. Granville laments the evil of which I complain, and gives some frightful examples, but without suggesting, as it seems to me, a sufficient remedy. (See his Report of the Practice of Midwifery at the Westminster Dispensary, page 201.)

the accident might have been prevented ; and after the occurrence of the rupture, the delivery of the fœtus and replacement of the intestines would have given the woman a much better chance of her life than she ultimately had, by the removal of the two principal causes of the violent abdominal inflammation discovered after death, namely, the contact of a foreign solid with the delicate peritoneal covering of the intestines, and the constriction of so many of their folds by the edges of the lacerated opening. As long too as the uterus was prevented from contracting, more or less of bloody effusion was allowed to go on, which must necessarily have added to the inflammation. The laceration was not, it is probable, so extensive originally as it here appears, but must have been materially augmented by the continuance of a great portion of the fœtus between its lips for the long period of seventy hours.

Pelvic deformity appears to have contributed to produce this laceration, and to have been of a comparatively recent development, as it is probable that had any of her previous labours been difficult, she would have mentioned the circumstance, which it is believed she did not. Such deformities some authors suppose to be a frequent cause of this accident.* The phenomena, well marked, were followed by those of inflammation, to which succeeded symptoms of intestinal strangulation.

Writers state the fatality in uterine and vaginal lacerations, to depend on three causes, viz. collapse, hæmorrhage, and inflammation ;† of these the last appears to be the most general,‡

* Robertson in *Edinburgh Medical and Surgical Journal*, vol. xlii. p. 49.

† Ingleby in *Dublin Medical Journal*, No. XXIV. p. 451.—Maygrier *L'Art Des Accouchemens*, tom. ii. p. 271, &c.

‡ “ In every case which I have seen, the intestines were chiefly affected, being much inflamed.”—BURNS' *Midwifery*, p. 495. “ If the patient survive the more immediate effect of the accident, symptoms of abdominal or peritoneal inflammation presently supervene, and are gradually progressive.”—RAMSEORHAM'S *Practical Observations in Midwifery*, Part 1, p. 387.

and most alarming, and as such should be the chief object of our attention.

I shall not occupy the time of the Society by insisting on the propriety of immediate delivery, such a practice being, I believe, generally approved of at the present day ; but I do think it expedient to say a word or two on the necessity of the subsequent employment of bleeding, both topically and generally, as an idea has prevailed that such depletions are often uncalled for, and may, therefore, safely be dispensed with.*

On referring to Doctor Collins's valuable observations,† I find that two cases which recovered had, the one six dozen, and the other nine dozen of leeches applied to the abdomen ; and that six other cases which were similarly treated, (two of them being also bled from the arm for convulsions,) held out to a longer period than the subjects of such untoward accidents usually do : some of them dying of accidental complications, as lumbar abscess, &c. In Dr. Ramsbotham's Observations,‡ three cases of recovery are given, in two of which venesection, and in the third leeches to the abdomen were employed ; in Doctor Frizell's case§ bleeding from the arm was used, and the subject of the present history probably had her life prolonged by a similar measure.

Truth, however, compels me to state that recoveries have taken place under opposite circumstances, or without the employment of such measures ;|| but in such instances the symp-

* " There is certainly little chance of any person surviving a rupture of the uterus ; it therefore might be doubted whether it would be more eligible to suffer the patient to die without giving her further trouble," &c.—DENMAN's *Practice of Midwifery*, p. 328, 4to.

† *Practical Treatise on Midwifery*, p. 248.

‡ *Opus citatum*, Part 2, pages 484 to 498.

§ *Dublin Medical Transactions*, vol. ii. p. 15.

|| *Duncan's Annals of Medicine*, vol. iii. p. 277. M'Keever in *Dublin Medical Transactions*, vol. iii. p. 280. *Memoirs Medical Society London*, vol. iv. p. 253, &c. Labatt in *Dublin Medical and Physical Essays*, vol. i. p. 348.

toms have been unaccountably, mild ; and I merely wish to add my conviction to that of more experienced practitioners, that, after delivery, to be effected in the manner best suited to the circumstances of the case, the chief danger to the patient, in the majority of instances, will arise from abdominal inflammation ; and that the more promptly such symptoms are met, the greater chance will be afforded to the patient of recovery.

The next preparation displays an extensive ulceration of the uterus ; the cervix and the os uteri have been completely destroyed, and the ulcer occupies half the remaining portion of the organ. Inferiorly it is seen to extend one-third upon the vagina all round, being at one part quite superficial, as if a portion of the lining membrane had been but just abraded.

The surface of this ulcer had the white, yellow-greenish aspect of a scrofulous sore, and it discharged a copious thick curdy secretion : to the touch it gave the sensation of an irregular border, extremely soft, and easily breaking down.

The uterus (at least what remains of it) is seen enlarged to twice the natural size, or to about its ordinary bulk at the second month of impregnation. Its upper third retained a natural consistence, but the middle was somewhat firmer than is usual ; the vascularity of this portion of the organ was much increased, and its texture, on being cut into, resembled that of an impregnated uterus, the walls being from half an inch to three-quarters in thickness. There was also a distinct line of demarcation between this part and the superior or more healthy portion of the organ, which occupied the entire thickness of the walls. When the finger was passed into the vagina, it readily entered the uterus through a large relaxed opening, surrounded by a soft irregular border.

In regard to the uterine appendages, the tubes and ovaries were nearly normal, the latter, however, of a size less than usual, and a small tubercle was developed in the left. The disease had not extended either to the bladder or rectum.

The abdominal viscera were healthy, with the exception of

the liver, upon the convex surfaces of which marks of former inflammation were evident.

The thorax, unfortunately, was not examined.

The body was plump and loaded with fat, which also occupied the abdominal cavity. The left lower extremity and corresponding labium pudendi were anasarcaous.

The subject of these observations was aged 40, of middle size, and stout make, and the mother of eight children; she had been attacked six months before with hæmorrhagia, which continued profusely and almost daily, for five months, when it ceased, and was followed by the anasarca and great exhaustion, but without pain at any period.

When I saw her she was moribund, her countenance anemic, like that of a person who had lost a large quantity of blood, she spoke with difficulty, appeared greatly exhausted, and survived but four hours. Under such circumstances no vaginal examination was instituted.

What was the nature of this sore? Scrofulous, I reply, well marked! indeed so clearly characterized, as to be beyond reach of being confounded with any other species of ulceration!! It may be said that it was nothing more than an instance of the corroding ulcer so well described by Dr. Baillie* and Sir Charles Clarke;† I admit a resemblance in some respects, but there are differences in others;‡ at the same time that I object to the employment of the term "corroding ulcer" altogether, as both unscientific and inexpressive. What ulcer is there which is not corroding? in fact all ulcerations are nothing more in common language, than corrosions of the parts which they

* *Morbid Anatomy*, vol. ii. p. 337.

† *Diseases of Females*, vol. ii. p. 185.

‡ The time of life of the patient, the extent of the destruction of parts, and the profuseness of the hæmorrhage, coincide with Sir Charles Clarke's description; but there never was any burning pain in the present instance, "as if a hot coal were within," and we find an enlargement and thickening of parts, of which he does not admit the existence.

attack. I can conceive a corroding ulcer of the uterus to be scrofulous, syphilitic, or cancerous in its nature, without losing its characteristic peculiarity, but should be glad to see a more physiological designation adopted for it.

Scrofula in the form of ulceration has seldom been observed to affect the inferior portion of the uterus, and this part of our system seems but little obnoxious to its attacks :* Andral, however, notices an old woman dying of phthisis, in whom the uterus was found full of pus and lined with a false membrane ;† and Louis reports the case of a woman, aged twenty-four, also dying of phthisis, in whom the interior of the uterus and upper half of its neck were of a dull yellowish colour, and studded with tubercles.‡ Two similar instances are given by M. Renaud, also occurring in phthisical females, aged thirty-nine and forty-five years, in one of whom numerous ulcers existed in the vagina, and in both the uterus was enlarged with tuberculous matter in the fallopian tubes.§ Madame Boivin too, in her valuable work, gives us the case of a girl, aged sixteen years, constitutionally scrofulous, and dying, like the others, of consumption, in whom both ovaries were tuberculated, and the uterus lined with a white granular matter, spongy and softened.||

Such particulars, added to the numerous facts generally diffused in M. Louis's interesting work, as to the coexistence of

* "Il est rare d'ailleurs que ce viscère présente des signes locaux pour faire reconnaître cette affection."—*Nauche Maladies Propres aux Femmes*, tome ii. p. 531.

† *Anatomie Pathologique*, tome ii. p. 681.

‡ "La cavité de l'utérus et la moitié supérieure de son col étaient d'un blanc jaunâtre, avaient uncoup d'œil mat et une surface inégale ; ce qui provenait de la transformation de leur couche superficielle en une matière tuberculeuse tres-ferme d'une ligne d'épaisseur environ. Au dessous de cette couche qui formait un plan non interrompu se trouvaient encore des granulations miliaires de la même nature."—*Recherches Anatomico-Pathologiques sur la Phthisie*, p. 405.

§ *Archives Générales de Médecine*, tome xxvi. p. 493.

|| *Maladies de l'Uterus et de ses Annexés*, tome i. p. 307.

scrofula in several of our tissues, at one and the same time, afford but little encouragement to our hopes of cure in such cases; nevertheless the usual constitutional treatment, together with the local employment of the preparations of iodine, might occasionally be found advantageous, and therefore would at least deserve a trial.*

The third preparation displays an example of the large, white, fleshy tubercle or fibrous tumour of the uterus, combined with a polypous growth from the inner surface of the fundus. The uterus is seen to be generally enlarged, probably as much so as at the fourth month of pregnancy, and this enlargement is found to depend on hypertrophy of its tissue above and in front, from the development in those situations of fibrous matter. On cutting through this enlarged part a considerable polypous growth was discovered, irregularly lobulated and fissured, and growing, by a broad basis, from the fundus and one side of the body of the uterus. This tumour is softer and less firm than the general uterine growth, though they may be nevertheless, identical in structure.

As this case did not occur in my own practice, I am unable to give a very accurate account of it; thus much, however, I can state, the woman was advanced in life, say fifty years of age, and had laboured under a morbid growth, impeding the due evacuation of the bladder and rectum, for at least two years. On examination *per vaginam*, the os uteri was not to be felt, being supposed to have been displaced by a tumour

* Since these remarks were put together I have discovered three or four preparations in our museum, of scrofula affecting the uterus or its appendages; in the most conspicuous of these the disease appears in the form of tumours filled with soft matter situated in the uterine substance, and combined with hepatic disease of a like nature, in the same subject; in the others the tubes or ovaries are occupied by similar productions. In the same collection is to be seen the uterus of a Barbary ape, occupied almost entirely by a large softened tubercle; and similar affections have been noticed, along with phthisis and other forms of scrofula, in several of the other varieties of the quadrumani.

which had descended between the bladder and rectum ; true it was such a tumour existed, but it was not at the time discovered to be incorporated with the uterus, but on the contrary considered as a distinct growth, probably capable of removal by a surgical operation ; the woman, however, gradually declined, and died before the operation was decided on.

At the *post mortem* examination “the uterus was found retroverted and pushed down, so as to press on the perineum, and had formed adhesions with the surrounding parts ; its bulk was nearly that of the head of a full grown foetus, its walls were thickened to about three inches at the fundus, and it contained a dark-coloured fleshy excrescence of the polypous kind, but with a broader base and irregular ; one of the ovaria was occupied by a small fibrous tumour.”

I consider this to be an interesting specimen of morbid uterine structure, in two points of view : first, as an example of the fibrous growth from two distinct portions of the uterus at the same time ; and secondly, as an instance of retroversion induced by causes independent of pregnancy. Authors* have usually confined the fibrous growths of the uterus to three situations, viz. the external surface under the peritoneum, the internal surface beneath the lining membrane, and the substance of the organ between the muscular fibres ; or they may occupy all or any one or two of these positions together, as in the case before us. Most generally their seat is confined to some one place, perhaps we might say that those growing from the inner surface are most frequently met with, and it is fortunate that such is the case, as they are necessarily more within reach than when developed in other situations.

A question here arises, are we, in cases where fibrous tumours (or polypi), growing from the inner surface of the uterus,

* Dupuytren, *Leçons Orales de Clinique Chirurgicale*, tome iii. p. 454. Boivin et Dugès *Maladies de l'Uterus et de ses Annexées*, tome i. p. 301. Desault, *Œuvres Chirurgicales* par Phil. Jos. Roux, tome iii. p. 378, &c.

produce such symptoms as to demand an operation, but are, at the same time, complicated with similar growths on the outer surface or in the uterine substance, to take the ordinary measures for their removal; or are we not to interfere, on the principle that it is useless to remove disease from one part, when it must necessarily soon appear again in another?

On this subject some of our highest authorities are at issue, for while the late Doctor Gooch* and Professor Roux† advocated the latter practice, the former was clearly and strenuously insisted on by the late Baron Dupuytren,‡ one of the highest authorities, it must be allowed, on this as well as most other surgical subjects. The reasons which the Baron assigns I consider to be cogent; “you excise such polypi,” says he, “*because* you thereby remove the local inconveniences arising from their bulk, pressure, discharge, &c., and prevent their possible degeneration into cancer, to which they are very liable; and *because* in the other situations they are often extremely indolent, of a very slow growth, becoming sometimes bony or cartilaginous, and seldom shortening life.”

Let us now turn to the case as one of retroversion. To produce this displacement writers appear to be agreed in assigning one necessary condition, viz., that of enlargement,§ to which some|| add another, that of relaxation of the ligaments. Such enlargement must take place to a certain extent, and not be in excess.¶ The production of retroversion by uterine polypi

* Diseases of Women, p. 279.

† Œuvres Chirurg. de Desault par Ph. J. Roux, tome iii. p. 382.

‡ Leçons Orales, tome iii. p. 564.

§ Burns' Principles of Midwifery, p. 249.

|| Boivin et Dugès Maladies de l'Uterus, &c., tome i. p. 137.

¶ “The retroversion of the uterus has generally occurred about the third month of pregnancy, and sometimes after delivery; it may likewise happen when the uterus is, *from any cause*, enlarged to the size it acquires about the third month of pregnancy, but not with such facility as in the pregnant state, because the enlargement is then chiefly at the fundus. If the uterus be but *little enlarged*, or if it be

has been noticed by Madame Boivin* and Sir Charles Clarke,† but I am, in the present instance, more disposed to attribute the occurrence to the fibrous tumour of the body, particularly as it was unusually dense and heavy.

Other sorts of enlargements of the cervix and body of the uterus, whether inflammatory or not,‡ have been known to give rise to retroversion, such as those produced more immediately by exposure to cold and imprudences of various kinds, whether some time after delivery before the uterus has returned to its ordinary dimensions, or as a consequence of suddenly suppressed menstruation, including, perhaps, what may be termed a temporary hypertrophy of this part of the reproductive system.§ Certain states of the neighbouring organs, as the bladder and rectum, may also act as exciting causes, from the mechanical pressure to which they give rise.||

In most cases of retroversion unconnected with pregnancy, it will not be necessary to adopt manual efforts for replacement, the removal of the exciting causes, and attention to the condition of the neighbouring hollow viscera being sufficient;¶ but should

enlarged *beyond a certain size*, it cannot well be retroverted: for, in the first case, should the cause of a retroversion exist, the weight of the fundus would be wanting to produce it; and in the latter the uterus would be raised above the projection of the sacrum and supported by the spine."—DENMAN'S *Midwifery*, p. 96, 4to edition.

* Opus citatum, tome i. p. 138.

† Diseases of Females, vol. i. p. 273.

‡ Pearson on Cancer, p. 113.

§ See an interesting paper by Mr. Robertson in *Edinburgh Medical and Surgical Journal*, vol. xviii. p. 520.

|| A case of retroversion fatal from obstipation and vomiting is given by Dr. Marcet in Sir A. Cooper's work on Hernia, vol. ii. p. 63.

¶ "But in the unimpregnated state the symptoms never arrive at this melancholy height, and for this plain reason that the uterus in such cases never acquires so much size as to entirely and intimately occupy the lower portion of the pelvis, and consequently cannot completely obliterate the urinal and fæcal canals."—DEWEES' *Essays on various Subjects connected with Midwifery*, p. 263.

such measures be thought proper they ought at once be taken, as the uterus may become adherent in its unnatural position, as occurred in the present instance, and in another related by Dewees.*

I am not aware that a retroverted uterus has hitherto been in danger of removal by operation, but can conceive the adoption of such a measure with impunity to the patient, (particularly if beyond the period of child bearing,) as has been done in certain cases of inverted uterus mistaken for polypi;† at the same time that I would not go so far as to recommend such a practice unless under very peculiar circumstances.

The next preparation exhibits a portion of a fibrous polypus removed from the cervix uteri, possessing an unusual degree of vascularity, so as to endanger the life of the patient, at the time of its removal, and thus becoming worthy of our notice.

On the cut surface may be observed the orifices of two arteries, the larger nearly the size of a crow quill, being the sources which furnished the blood.

Excision by means of a large scalpel was the mode of removal employed, the forceps of Museux being used at the same time to bring down the parts as far as might be. A most profuse hæmorrhage instantly followed, which was so alarming as to demand the instant insertion of a plug in the vagina; an old soft silk handkerchief was used with the desired effect; this, in half an hour, was removed, but the bleeding instantly recurring with unabated violence, a fresh plug was inserted with the same effect in promptly arresting it.

The subject of the case was a stout woman, aged thirty-nine, unmarried, who had been employed as a farmer's servant.—Eight years before she had had menorrhagia, lasting pretty constantly for two years, and succeeded, three years afterwards,

* Dewees' *Essays on various Subjects connected with Midwifery*, p. 263.

† Johnson, in *Dublin Hospital Reports*, vol. iii. p. 481.

by retention of urine, followed by paralysis of the bladder and amenorrhœa. About a year since I examined *per vaginam*, when there appeared to be mere uterine enlargement with a partial closure of the os uteri, and it was not for several months afterwards that the vaginal tumour became perceptible, during a sudden exertion in lifting a weight.

When this woman was admitted into Saint Mark's Hospital, in September last, the tumour had descended to within two inches of the *orificium externum*, its upper part being attached by a broad semicircular base to the left side and front of the cervix; the os uteri appeared to be dilated, and considerable bloody discharge followed all examinations of the tumour, although no such appearances were observed at other times.

She had also, at this period, and of three years' standing, a firm moveable tumour, about the size of a small orange, on the right side of the abdomen, half way between the spine of the ilium and the umbilicus, occasionally the source of much pain and suffering; and a small soft polypus growing from the left side of the vagina.

At the time of the operation, probably from the existence of adhesions in connexion with the abdominal tumour, the cervix uteri could not be drawn down by the hooks, as far as could be wished; from which cause, as well as the unexpected hæmorrhage, little more than one-half of the tumour could be removed. The existence of disease in the body of the uterus or in the ovarium being also demonstrable, and the perfect recovery of the case, under such circumstances problematical, no farther attempts to excise the remainder of the tumour were considered to be advisable.

The vaginal polypus (which appeared of a cellular nature) was torn away, and she was dismissed considerably relieved.

In this case we had the co-existence of the cellular and fibrous kinds of polypi, both growing together from the same species of mucous membrane; and the principal tumour appears

to have been attached immediately within the os tincae, one of the lips of which had become elongated. Whether a second polypus was developed at the same time within the uterine cavity, as has sometimes occurred,* I shall not pretend to say; neither did I consider myself justified to make particular examination on the subject, both from the hæmorrhagic disposition which existed, and from the relief which had been already experienced.

The presence of the abdominal tumour also complicated matters; whether it was a pediculated fibrous growth from the body of the uterus, or an ovarian tumour, could not be determined, and although the woman was exceedingly desirous to submit to any operation for its removal, and notwithstanding the encouragement held out by the several instances of successful result detailed in various periodicals;† I did not consider that her sufferings from this cause were sufficient in degree to authorize me to lay open a serous cavity of such extent, and so disposed to inflammation, as the peritoneum.‡

This is but the fourth case of alarming hæmorrhage supervening on the excision of a fibrous polypus, which I have been able to find on record. The first is stated to have occurred in the olden time to Zacutus, and to have proved fatal to his patient; but I have not met the details: the two others are recorded by Dupuytren; § in these the bleeding was speedily stopped by the "tampon," but one of the individuals perished on the twenty-fifth day of peritonitis.|| I firmly believe that our

* Dupuytren, *Leçons Orales*, tome iii. p. 432.

† *Eclectic Rep.* vol. vii. p. 242. *Edinburgh Medical and Surgical Journal*, vol. xviii. p. 532. and vol. xxii. p. 247.

‡ Since this paper was written, the woman has died, but whether of her old complaint or not, I could not ascertain.

§ *Leçons Orales*, tome iii. p. 450.

|| "A loss of blood" (observes the Baron, talking of some of these cases) "more or less considerable, is always useful after these kinds of operations, and renders much less imminent the consecutive inflammatory accidents to which the patients

patient would have run the greatest risk under the employment of ordinary measures.

Polypi have occasionally been observed to be highly vascular, nay, large blood-vessels have been seen ramifying on their surfaces,* such facts, however,† are no proofs that their pedicles, when divided, may furnish blood, as that is generally not the case; but they have their utility, as they point out to us, at least, that the division of the pedicle of the polypus is not always an operation of such little importance as some practitioners affirm.‡

Hæmorrhage may also occur under other circumstances, hitherto not much noticed, viz. at the period of the falling off of the ligature, when used for the constriction of polypi; thus M. Dubois gives us the astounding information, that at such a period he has many times seen blood issue forth in abundance, and has lost in this way many patients.§ I am happy to say, that I am not aware of the occurrence of any such cases in this country.

So much difference of opinion exists as to the greater or lesser eligibility of the two methods generally employed for the removal of uterine polypi, viz. excision and the ligature, that I shall not enter into the question, but simply express my conviction, that both methods occasionally prove fatal, and much more frequently than has been supposed, more especially when the tumours are of a large size; such fatality appearing to arise

are exposed.”—*Leçons Orales*, tome iii. p. 459. But on his own shewing such salutary discharges have seldom occurred to him, such polypi as he had excised in general affording but a very few drops of blood.

* Saviard (*Obs. Chir. Obs. 36*) mentions one with two arteries and two veins; the *Journal de Medecine*, tome xxix. 1768, notices a second with two arteries and one vein; and a third, with pulsations in the pedicle, is described in *Mem. de l'Academie de Chirurg.*, tome iii. p. 533, 4to.)

† *Leçons Orales*, tome iii. p. 459.

‡ Boivin, *Maladies de l'Uterus*, tome i. p. 340.

§ *Dictionnaire des Sciences Medicales*, Art. Polypes.

principally from three causes, viz. inflammation of the pelvic viscera, uterine phlebitis, and low fever.

The pressure of a large polypus upon the parts filling the pelvic outlet, often produces a chronic inflammation of the uterus, small intestines, &c., which only requires the additional irritation of an operation to develop a more acute and fatal form of it: under such circumstances a mere manual examination of a polypus made more forcibly than usual, has determined a fatal inflammation.* Uterine phlebitis, in consequence of operations upon polypi, is more rare, but still fatal cases are not wanting. By the term low fever, I understand, that state of prostration and exhaustion, preceded by rigor, into which patients with large, sloughy suppurating polypi are apt to fall, and of which they die at the end of six or eight days: sometimes marks of inflammation are found, sometimes they are absent, the patients appearing to be carried off under the operation of an animal poison.

The doctrine of the absorption of pus as a cause of fever will not, I know, be generally admitted, nor have I any thing new to offer on the subject, I therefore refer my readers to Baron Dupuytren's valuable monograph on fibrous tumours, in which he tells us of twelve or fourteen patients actually poisoned by putrid absorption.†

ART. XXI.—*Medical Problems*. By WILLIAM GRIFFIN, M.D.,
Limerick.

(*Continued from Vol. XI. p. 17.*)

Is Laryngismus Stridulus, or the crowing disease, a spasmodic or paralytic affection?

Before entering on the discussion proposed in the foregoing problem, or again opening a question which seems to have

* *Leçons Orales*, tome iii. p. 494.

† *Ibid.* p. 559.

been almost decided by the very ingenious and clever work of the late Dr. Ley, it may be of use to give a sketch of the few cases of laryngismus stridulus which first attracted my attention to the subject.

The complaint does not appear to be so remarkably rare of occurrence, as was imagined when it first came to be accurately described by medical writers. The cases I shall relate all happened within the last two years in my own practice, and the great majority of the profession who have written on the diseases of infants latterly, evince a familiar acquaintance with it. The fact that an affection, I might almost say so common, should have been completely overlooked by almost all the eminent men of past times, is sufficiently mortifying, whether looked upon as illustrative of the difficulty of the diagnosis, or a general inaccuracy of observation.

I shall first describe two cases in which the phenomena of the crowing disease formed but a minor or less important part of the whole affection. They are curious as illustrations of a complaint, which, I believe, has not even yet been noticed by any medical writer, and which might, perhaps, to distinguish it from the crowing disease, be appropriately called the *crowing apoplexy of infants*.

An infant of rather a spare and puny frame, although healthy looking in its countenance, on the eleventh day after birth, was affected with bowel complaint. The motions were very fluid; of a light yellow colour, and there were about six in the course of the day. It got a little chalk powder from the nurse at night, and next morning a drop of laudanum. The motions were less frequent, though still loose, and the child did not look well. It had been fed on milk and water, and a little prepared barley from its birth, the flatness of its mother's nipples preventing its obtaining much nourishment at the breast; and it was now suckled by a woman whose child was twelve months' old, but perfectly healthy, while they were waiting to procure a younger nurse. In the evening, however, it was seized sud-

denly, as if with suffocation, losing its breath, and becoming first pallid, afterwards dark or purplish in the face, and finally, when all respiration was suspended, of a death-like hue. There was a stiffening of the frame and twitching about the mouth, and the thumbs were drawn into the palms of the hands ; but there was no convulsion of the body or extremities. After a longer or shorter interval the breath was recovered by gasps, which were accompanied by a crowing sound, that became louder after a time, so as sometimes nearly to resemble hiccup. This crowing again gradually died away, and the respiration became easier, but always recurred on the approach of another fit, and continued until the breath was lost ; when the child, as in the first instance, grew dark for a moment or two, and then pale and death-like. It was indeed sometimes impossible in these intervals of suspended respiration, which recurred frequently, to say whether the child was living or dead. It lay cold, white, breathless, and without sign of animation, often for three or nearly four minutes, counted by a watch, and then recovered with a faint gasp, followed by the crowing. The intervals between the fits of suspended respiration were seldom longer than half an hour, although they occasionally extended to an hour or more. The crowing sometimes continued throughout the interval, at others abated for a little. From the first moment of the attack the infant never recovered its consciousness, the pupils seemed fixed, the eyes senseless, and all power of deglutition was lost. Whenever a teaspoonful of liquid was given it remained in the mouth or flowed out at the corners, or if it went back to the larynx it obstructed the breathing, and brought on the fit of suspended respiration. The only approach to sensibility observable at any time was in the slight motion of the lips, which were sometimes seen to work as in the action of sucking. There was rarely, when the fit of suffocation commenced, a slight convulsion or twitching of the muscles of the face, which, however, never lasted longer than the blackness or darkness, and seemed in fact a struggling for breath ; but as the case

became protracted, although the crowing in the intervals continued as loud as before, no darkening of the face or writhing of the features preceded the fit.

As the power of swallowing was gone, the treatment was confined to injections, stimulating liniments to the spine and stomach, and the warm bath. The first consisted of starch and assafoetida. Turpentine was afterwards used, and when these means seemed unavailing, laudanum was administered. The injections were sometimes retained for half an hour, but usually came away soon, and in no case had any perceptible influence in preventing the fits, although as much as eight drops of laudanum were given in this way. The bath at first seemed of service, and prolonged the interval, but after some repetitions lost its effect. A blister, which was applied to the back of the neck, was equally fruitless, and after an illness of about forty hours' duration, the poor little sufferer ceased to breathe any more.

CASE II.—A fine, round-limbed, healthy-looking boy was born after a favourable labour of about six hours' duration. He got castor oil, as infants usually do, and on the morning after I was requested by the nurse to inspect the motions, which were very green; the one she shewed me had the appearance of chopped spinach. I directed a grain of calomel to be given, and if it did not move the bowels freely, the oil in the morning. Next day the motions still continuing green, though less so than before, the calomel was repeated. On the fourth day the bowels were much better, and on the fifth the evacuations seemed to be of the ordinary character. The child during this time got nothing except the breast, and (the mother not being able to nurse long nor often on account of sore nipples) a little prepared barley, or milk and water. It appeared to thrive, and did not lose its plumpness, but the mother afterwards informed me it used to start frequently with a cry or scream out of its sleep, in a way she had never observed with her first child, who was alive and well. After this sudden start or scream it usually

fell asleep immediately. On the evening of the tenth day, when my attendance had terminated, the child was attacked with slight complaint in the bowels, for which it got a little chalk mixture. The bowels were moved five or six times during the night, not very profusely ; and the evacuations were of a pale yellow colour. At eight o'clock the next morning I was summoned hastily to visit it, and on entering the room, to my utter consternation, I found it gasping, after a fit of breathlessness, precisely similar to that described in the former case, and followed by the same crowing noise in inspiration. It was perfectly insensible ; the pupils of the eyes were natural in their appearance, but sluggish in their movements ; the power of swallowing was gone ; the thumbs were bent into the palms of the hands ; the surface was pale and cold ; the impulse of the heart and the pulse at the wrist were feeble. The fit of suspended respiration occurred at intervals of perhaps twenty minutes, though they were sometimes longer. The child's countenance did not darken in the fit as in the other case, it became instantly pale on losing its breath ; the lips and even the tongue were cold, and when I put back my finger to the pharynx, to ascertain whether I could by the touch excite the action of swallowing, I found the parts motionless. The crowing continued almost through the whole of the interval, and, if it did cease, it always recurred on the approach of the fit. On one occasion, after the warm bath, it seemed to subside into a breathing slightly stertorous, which did not at all occur in the case of the former child. There was, however, once or twice, in the course of the day, the same working or sucking motion of the lips which I noticed in that case, indicating some faint approach to sensibility.

The treatment of this little patient differed in no respect from that employed for the former infant, except that no laudanum was administered, and it was equally unsuccessful. If it could be said that anything gave the least relief, it was the warm bath. A blister to the nape of the neck and vertex was

applied early, but before it could have had any effect, the little sufferer expired, having struggled altogether only six hours against the disease.

On examining the body of the first infant no appearance of disease was observable in the heart, lungs, or bowels. On opening the head there was excessive difficulty in detaching the skull from the dura mater, and as soon as it was removed the hemispheres fell asunder in a diffuent or pultaceous mass, so that it was impossible to make any regular examination. Having removed the gelatinous mass of brain and cerebellum, the medulla oblongata and spinal cord were found of healthy consistence.

I now thought I had a clue to the explanation of the symptoms. There appeared to be here sufficient disorganization to account at least for the insensibility, and attach probability to the supposition that the crowing respiration was dependent on some affection of the brain. Billard speaks of such general ramollissement, as often occurring immediately after birth, and mentions that it is then more considerable and extensive than at any other period of life. He thinks it probable that it sometimes begins even before birth. I do not know what the attending symptoms were, but in ten such instances in which the softening extended to the whole of the spinal cord, he relates, that the respiration was laborious and imperfect, the limbs flaccid and motionless, and the pulsations of the heart scarcely perceptible. These symptoms were all absent in the case of the infant which I have detailed, it would seem, because there was no lesion or disorganization of the spinal cord or medulla oblongata; but the functions of the hemispheres and of the cerebellum were altogether suspended, as the ramollissement of these parts would lead one to expect.

There could, however, be no stronger proof, that identity of functional derangement is no evidence of the identity of the lesion which produces it, than appeared on examining the body of the second infant, the symptoms of whose disease so closely

resembled those of the first. The brain seemed perfectly healthy, and there was no sign of disease in any other organ that I could detect. The examination in either case was a very hurried one, which precluded any examination of the state of the eighth pair of nerves, or recurrents, or of the bronchial glands. Indeed it did not occur to me as a matter of any importance to make the examination at the time, as I thought it utterly improbable there could be any scrofulous enlargement of these parts immediately after birth. There certainly was no perceptible enlargement of the glandulæ concatenatæ, or other glands in the neck. The thoracic and abdominal viscera were also in this case healthy. I do not know, however, what importance, if any, to attach to one circumstance: the cardiac orifice of the stomach was found plugged up with a firm coagulum of milk, which retained the exact shape of the parts when it was removed.

The complaint which I have described as affecting these two infants, though closely allied to laryngismus stridulus, or crowing disease, is obviously very distinct in its nature, or more truly perhaps in the amount of nervous matter involved in the morbid action. Possibly the same difference may exist between them, as between apoplexy and local convulsion, or, as Dr. Ley suggests, palsy. Whatever it may be, the distinction between them is very marked. In laryngismus there is no insensibility, no crowing, except immediately on recovering from the fits of breathlessness, no apparent illness whatsoever in the intervals, no pallor or coldness of the surface, or feebleness of the action of the heart, or of the pulse at the wrist. In the affections which I have just described, on the other hand, there was, from the moment of seizure, an utter unconsciousness and insensibility both in the fit and interval, so much so indeed that even a cry did not escape either of the infants, except in one instance, when the first was plunged into a hot bath; there was an incapability of swallowing, excessive feebleness of the heart's action, and coldness of the surface of the body. In the second

child, there was at one time, when the crowing subsided during a longer interval than usual, some approach to stertor in the breathing. I ought perhaps to notice one other distinction between this and the crowing disease of Dr. Clarke and others, that it did not terminate in general convulsions as the latter usually does.

It is exceedingly difficult even to speculate with any probability on the cause of this remarkable affection, or to connect it with any certain pathological condition. From its occurrence a few days after birth, at which period, Billard states, general *ramollissement* of the nervous centres is most common, I should have been disposed to connect the symptoms with some such disorganization, if, in the second case, the brain had not been found apparently healthy; and I should have attributed them to some congenital defect, which the weak appearance of the other infant might in some degree countenance, only that no suspicion of the kind could be entertained in reference to the second. It was born a strong, plump, round-limbed little fellow, in every way as promising for long life as one could desire. It might, indeed, be said that the very earliest evacuations were unhealthy, and the sudden screams out of sleep, described by the mother, indicated something wrong from the commencement; but these symptoms are of every day occurrence with other infants who go on well notwithstanding.

From a fair consideration of both cases it becomes a question of great interest, whether the complaint might not be one of mere functional derangement, as we are well assured many cases of fatal convulsions are. It was in both instances preceded by disorder of the bowels, and on the evening previous to the attack by diarrhœa. Supposing it to be a functional affection, to what cause are we to attribute it? An experienced practitioner informed me he had met with similar cases which also proved fatal, and that he believed them to depend upon retention of the meconium. In the first case narrated, however, castor oil was given to the infant immediately after birth in the usual manner, and repeated as occasion seemed to require; and in the second,

calomel and castor oil were given until the evacuations assumed a natural appearance. Could the hard plug of coagulated milk, found in the cardiac orifice of the stomach of this child, by possibility produce such a frightful affection of the nervous centres?

I now proceed to the cases in which the question at issue between a portion of the profession and the late Doctor Ley is more directly involved. They accurately agree with those already published by that gentleman and others, and are given in detail only because the amount of those already upon record is far too slight to admit of safe general inferences as to their pathology or treatment. In all diseases, the treatment of which is difficult or obscure, the numerical system of deduction so successfully adopted by Louis in affections of the lungs, is the only one worth our attention, and without a large number of cases this cannot be resorted to, even in the imperfect manner, which these, when given loosely by different individuals, may admit of.

A fine, stout, muscular little fellow, at the age of seven months, was seized, in the nurse's arms, with sudden suspension of the breath, but after a slight struggle, and gasping attempts at inspiration, accompanied by a crowing sound, he in a few moments recovered. As the fits recurred two or three times in the course of the day, and occasioned a very just alarm to the parents, I was requested to see the child, whom I found playful and smiling on the nurse's knee before me. From the description of the fit, as it was called, I had no doubt as to its nature. My acquaintance with it having been chiefly derived from Dr. Clarke's communications on the subject, I entertained his views of its cerebral origin, believing that in this case the brain had become affected from the irritation of teething in a naturally plethoric habit. The gums were therefore lanced, active purgatives were given; and the child, who was a great feeder, was restricted to a lighter diet. The complaint recurred slightly for some days, when one or two teeth appearing, it ceased altogether, and for five or six weeks the child continued perfectly well.

It was then, however, attacked with a more violent fit than before. As well as I can recollect (for I kept no notes of the case) I then lanced the gums again, blistered behind the ears, and directed some purgatives. The complaint recurred frequently when the little fellow laughed or cried, and sometimes he awoke out of sleep with the gasping and crowing. He passed some days entirely without an attack; on others he had one or two. I now directed assafoetida and antispasmodics for him; but as there was no satisfactory amendment, the fits still returning occasionally in a very alarming manner, a consultation was proposed with a physician of eminence and experience. This gentleman took altogether a different view from mine of the disorder, and I saw evidently that he had either never seen it, or never distinguished it as a specific affection. He spoke of the crowing as singultus, and attributing it to acidity of stomach, with derangement of bowels, recommended that milk and its other usual diet should be laid aside, and broth substituted; a carminative mixture with rhubarb and magnesia was also directed. There was, however, no improvement, and on the second day after the child fell into convulsions. The face was very much distorted, and the convulsions were confined to one side, the other appearing to be palsied. Three leeches were applied to the temples, and the child was put into a warm bath. After a second consultation four leeches more were applied, and the little patient at length recovered, but was hemiplegic. The paralytic affection, however, disappeared, after the application of a blister to the nape of the neck and vertex. The improvement, though wonderful, was however temporary; the fits of crowing returned on the next day, and in two or three days, the little sufferer fell into convulsions again, accompanied with profound coma, from which he never recovered.

The mother had another child in the following year, which fell into convulsions in, I believe, the second month, and died. I did not see it, but mention the circumstance to show the family predisposition to such affections.

Some months after my attendance on the foregoing case, I was sent for by a lady who had lost many children, to prescribe for an infant that had been just seized with convulsions. She had only this little one, of seven or eight months old, and another, a weak, emaciated boy, aged four or five years, who was suffering with paralysis of the lower limbs from spinal disease, out of a family of eight children, most of whom had died of convulsions or hydrocephalus. The child was in a warm bath, and recovered from the fit when I had arrived. It was like the former, of a gross habit, and though still at the breast fed largely. It took bread and milk often, was allowed broths, and sucked meat greedily. It certainly did credit to the diet, for it was one of the finest children I ever saw, large, round, plump, and rosy faced, with eyes full of light and intelligence, and a disposition full of play. I found it had been attacked in a similar way about three weeks or a month before, and that a scabby eruption, with which the whole head and upper part of the face had been covered, was then first observed to decline. It had been teething for some time, and used to dribble a great deal, but this drain of saliva had also lately diminished. The convulsions, as described to me, were general. As the child was very plethoric, three leeches were applied to the head, the gums were lanced, he was freely purged, and small blisters were applied behind the ears. On the following day, although there was no return of the convulsions, he had once or twice, as the attendants described, a slight fit, though without spasms of the limbs. A more minute inquiry convinced me that it was an attack of the crowing disease. The child, they said, stiffened, and lost its breath; and although the crowing did not strike them particularly, when I imitated the sound and gasping manner of the infant, they at once recognized the perfect resemblance. I believe, indeed, one reason for the apparent infrequency of this disease is, that it is always spoken of as a convulsive fit by the nurse or attendants, and as the physician is seldom in time to witness the paroxysm, unless he institutes an accurate in-

quiry, and obtains a faithful and minute description of the attack, he necessarily confounds it with the common convulsions of infants. I was, indeed, for several days in attendance on this little boy, during which he had several returns of the fit, before I had an opportunity of verifying my conclusions, although from the mother's faithful picture of the fit, after her attention was directed to the importance of discrimination, I was perfectly convinced on the subject. The blisters were now kept open with savine ointment ; the bowels, which were exceedingly costive and obstinate, were regularly opened ; and he was restricted to liquid and farinaceous diet. In less than a week he seemed perfectly well.

On calling to see the child in about a fortnight after, I found him much pulled down ; his face pale, his look dull and depressed, and he had lost his cheerfulness ; there had been, however, no return of the crowing. Somewhat apprehensive of the effects of the debility which had followed the regular purging and restricted diet, I again allowed the little fellow broth, with some bread or panada ; in a few days he began to look up again, and before a week his former bright looks and playfulness had returned. About two months after this I was once more summoned to see him, and to my great regret found this fine boy had relapsed into a worse condition than he had been in any former attack ; the gasping and breathlessness recurred frequently, and were very protracted ; and the recovery of the breath, with the loud crowing, took place only after most distressing struggles. He was still teething, his bowels were very confined, and I lamented to see that the eruption on the head and face had nearly disappeared. The same treatment as before was once more resorted to ; lancing the gums ; the warm bath ; repeated doses of active purgatives, and enemata ; leeches were also again applied—these last remedies chiefly in consequence of the very plethoric habit of the child. When a sufficient degree of depletion had taken place, the fits still recurring at intervals of two or three hours, or oftener, blis-

ters were applied behind the ears, and injections with assafoetida were administered. On the next day a blister was applied to the nape, and a few drops of laudanum were added to each enema. Other antispasmodics were also administered by the mouth, but without evincing any influence in arresting the frequent attacks of the complaint. The laudanum was given two or three times in the injections, and did not produce any effect whatsoever, upon which I did not push it further. The little patient passed on the whole a better night than the previous one; but on the succeeding day suffered much, the attack occurring from the merest trifle, and attended with more appearance of convulsion. Towards noon he appeared stupid and heavy, and the pupils more dilated and sluggish in their movements. As the pulse was quick and feeble, the face pale, and the skin cold, I now gave some wine, and fed the little fellow occasionally with broth. An astonishing improvement followed the use of the wine, such, indeed, as I have not unfrequently witnessed in the advanced stage of hydrocephalus, where the great debility seemed to demand it, but invariably without leading to any permanent good. The insensibility and disposition to fall into a state of coma completely disappeared, the little sufferer looked up again with evident consciousness, and the fits of suspended respiration were shorter and less distressing. In two or three hours after, however, when I repeated my visit, I found all matters worse; the respiration was loud, and tracheal, and he seemed again insensible, though still recovering some degree of consciousness whenever the difficulty of breathing occurred. In this utterly hopeless state I was obliged to leave him to visit another patient who was in imminent danger, and learned afterwards that he died towards morning, convulsed.

The only symptom of convulsion which I witnessed throughout was the contraction of the thumbs in the palms of the hands. The first attack, the one with which the child suffered a fortnight or three weeks before I was called to see him,

and that on the day I commenced my attendance, which, however, I was not in time to witness, were both, I believe, instances of common convulsion. I had not read Dr. Ley's papers on this disease until a day or two previous to the boy's death, when I was induced to examine the state of the lymphatic glands particularly. *I found the glandulæ concatenatæ enlarged all down the neck*, but did not obtain permission for a *post mortem* examination.

A third case happened in the practice of my brother, who has given me the following account of it. "In the year 1835 M. W., a child a year old, began to be affected with derangement of bowels, white tongue, loss of appetite, and diarrhœa, attended with thin, gruel-like discharges of a pale colour; these attacks, which were at first attributed to difficult dentition, were usually got over in a week or ten days, but their repeated occurrence produced a degree of delicacy of look, and softness of fibre, and very much checked her growth. In the autumn of that year, being at the sea side, she got so severe an attack of diarrhœa, that for a day or two her life was despaired of; she came home in a very delicate state, with pallid look, white tongue, pale-coloured alvine discharges, which were now, however, of a healthy consistence, and she had lost her relish for all food except milk and thin gruel. In this state she was attacked with nervous startings, and occasionally also with what the mother called 'croupy breathing,' (laryngismus stridulus of Dr. Ley;) this last symptom was never so violent as to suspend the breathing entirely, or cause much distress. It was in other respects the loud sonorous inspiration so well described by Dr. Ley, always sudden in its onset, and passing off entirely in a minute or two. This state of things lasted some time, and during the continuance of it she was attacked with convulsions, which came on frequently during a day and a night, and which, after she had been blistered and purged, were at length subdued by an enema with fifteen drops of laudanum repeated once or twice. As the nervous startings,

the 'croupy breathing,' and the convulsions, were all considered connected with that irritable and over-sensitive state of the nerves which is often conjoined with debility, the occasional use of a sedative with tonics was recommended, and (a tonic mixture, with some tinct. opii, having been found to disagree, by interfering with the effect of the necessary opening medicine) the sedative was usually administered in the form of an enema, with fifteen drops of laudanum, whenever the startings or 'croupy breathing' shewed themselves. Under this plan, and by attention to the bowels, she passed six months without any return of the convulsions, and was rarely affected with the starting and 'croupy breathing,' which were always watched, and when they did occur subdued by the enema with tinct. opii.—She had a very capricious appetite all this time, the tongue usually coated or white, and for a considerable period she would take nothing but bread soaked in wine, on which regimen she gradually, however, gained some flesh and strength. I called at the house one morning, her bowels had been rather confined for some days before; she had a fit of the 'croupy breathing' in my presence, which was not a very violent one. When it was over, and the child was quiet again, the mother was expressing her apprehensions that this was premonitory of a convulsive attack, and asked me if I thought so. I gave an answer tending to quiet her fears, but had scarcely done speaking when the child had a violent convulsive fit, which came on without any of the 'croupy breathing,' and lasted nearly a minute. I put a blister on the nape of the neck, ordered some opening medicine immediately, and remained in the house three hours, during which time she had no return either of 'croupy breathing' or convulsion. I was obliged to go eighteen or twenty miles off that day, and on my return on the next, at noon, found the family in the utmost distress, the convulsions had returned repeatedly during the night, though the bowels had been well moved, and the blister had risen. The fits were growing more violent and very frequent, returning every half hour or twenty minutes. I

applied three leeches to the temples, gave the laudanum enema, and applied the stimulating dressing to the blister; the enema was repeated at intervals of about an hour, being usually given whenever a fit occurred of more than ordinary violence. After it was given five or six times it had an evident effect on the paroxysms, which though not lessened in frequency were very much diminished in violence, and indeed were now very slight. The child at this time had a very exhausted appearance, her face was pale, her pulse rapid, and there was so much insensibility that she could scarcely be got to swallow the drink which was occasionally put between her lips with a spoon. The insensibility was perhaps due to the opiate enemata, but as the convulsive paroxysms still returned, though not so violently, and the comatose state was one to which the case naturally tended, though no opiate had been given, I feared if it was allowed to continue, and my inference proved correct, the time lost would be irretrievable. The child was already blistered, and as new blisters would be too slow of acting, the only remaining alternative I had was to make an opening along the sagittal suture, down to the bone, and place a dossil of lint in it, wet with spirits of turpentine; she showed some sensibility to this by movements of the head from side to side; there was some little tendency to the fits for two or three hours afterwards, but they were so slight as scarcely to attract notice. The insensibility wore gradually off, and by the next evening she was able to take some milk and gruel, and recovered from all the effects of the attack, except the debility.

“The child lived eight months after this attack, but in an extremely delicate condition; her growth was checked; she showed a disposition to rickets in the limbs; acquired a double lateral curvature of the spine, and though nearly three years of age could neither speak nor walk. During this interval she had occasionally, but very rarely, returns of the ‘croupy breathing’ in a slight degree, and sometimes returns of the nervous startings, which symptoms were always attended to and subdued in the

usual manner when they did occur. I could never perceive in this child any appearance of swelled glands in any part of the neck during her illness, nor any where else except beneath the occiput, where some small ones could sometimes be felt, produced, probably by the discharge from the head, which was kept up to the time of her death. This event occurred after an attack of diarrhœa and vomiting, much slighter in degree than many that she had had previously. There was no examination of the body."

As the intimate pathology of these cases is likely I fear for a length of time to remain a mystery, it becomes a matter of extreme importance to ascertain in how far the pathology of the nerves in general and the inferences deduced from analogy may determine the general question whether they belong to the class of paralytic affections, and are dependent on the pressure of the recurrent nerves by lymphatic glands; or the convulsive disorders, and arise from some change occurring at the origin of the par vagum, affecting chiefly the superior laryngeal branches. The late Dr. Hugh Ley has already in a very elaborate work considered this subject in all its relations. He has indeed brought such a mass of information together in support of his views, and reasoned so ingeniously, that he has I believe made converts of a great body of the profession to his opinions, and left few disposed to question their correctness as regards some instances of the disease. There are, after all, difficulties which Dr. Ley has by no means satisfactorily got over, and which to my mind are sufficient to throw doubts on his whole hypothesis. I am not indeed, with my very limited experience of such cases, assuming to restore the opposite and formerly popular belief upon the subject, or to set the question at rest, anxious only to keep it still open to inquiry, and prevent the present practical influence of conjectures, which are problematical if not erroneous. Doubtful, however, as I am of the correctness of Dr. Ley's opinion, at least as it applies to the vast majority of cases, I cannot in common with the profession at large be insensible to

the advantages which must result to medical science from the philosophical and perfect manner in which he has brought the point at issue between him and former practitioners before the public, and the candour with which he has discussed it. It would indeed be a great gratification to me, as well as a help to our arrival at the true and just pathology of laryngismus stridulus, if a physician who devoted his mind to the inquiry more closely than any one before him, were still living to consider the few observations which I may here venture to offer.

The physiology of the parts engaged in the disease, and many facts illustrative of the general pathology of the nerves, give an extraordinary speciousness to Dr. Ley's views, which we shall not find so fully supported on a more minute examination. I shall, however, first consider what value should be attached to one or two symptoms, the presence of which, in the majority of cases, seems undeniable, and with respect to one at least is, in Dr. Ley's opinion, necessary as the exciting cause of the malady ; I mean eruptions on the scalp, ears, or face, and enlargement of the lymphatic glands in the neck or thorax. When we reflect how rarely the most alarming symptoms of the disorder, the suspension of respiration and crowing, are found in connexion with disease of the head, which is so common ; how impossible it is to account for that infrequency on any supposition of the affection originating in disease either of the superior laryngeal nerve, or of the origin of the par vagum ; and how perfectly it seems to be explained on the opposite view, as arising from pressure on the recurrent branches by enlarged lymphatic glands—when further we find that most cases are attended by eruptions about the scalp, ears, or face, with enlarged glands just in the course of the recurrents ; when we know that injurious pressure may possibly occur, the effect of which must be diminished power in the nerve ; and lastly when we are aware that the consequences of such defective power or paralysis must be difficult if not impossible respiration, we

must admit that a very specious case is made out in favour of the only hypothesis which offers a solution of the difficulties involved in the pathology of laryngismus stridulus. It is, however, worth while to examine the separate links of this imposing chain of reasoning.

It must strike every observer with regard to the enlarged glands, that as they are very common in children, as common almost as the disposition to struma, the crowing disease should be common also. Dr. Ley anticipates this objection, but he puts it only as applying to the immunity of adults from such an affection, which he accounts for by saying: 1st, adults are comparatively free from the causes which produce such enlargements; 2ndly, where the trachea has acquired the adult size, it gives more protection to the recurrent; and 3rdly, the larynx and glottis of the adult are much larger than those of the infant. These arguments, strong as they are in explaining why such an affection should rarely occur in an adult, by no means satisfy us as to his perfect immunity: amidst all the tumours to which adults are subject about the neck, it is rational, if Dr. Ley's hypothesis be true, to demand the citation of at least one fair case of laryngismus stridulus arising like it, resembling it, and terminating as it does in the infant. The real difficulty, however, is not about adults but about grown children, between whom and mere infants there can exist no extraordinary disproportion in the size of the trachea, larynx, or glottis; and in whom the disposition to enlargement of the lymphatic glands is, I should say, rather greater. Why are not grown children attacked with laryngismus stridulus? I have never seen the complaint except in mere infants; all Mr. Robertson's and most of Dr. Ley's cases appear to have been under two years of age; and Dr. Underwood speaks of it under the head of inward fits, as one of the disorders of early infancy.

The mere presence of enlarged glands in this complaint, may certainly, as Mr. Robertson of Manchester suggests, be a coincidence only. Dr. Marsh asserts that he has never seen

the crowing disease in any but infants of strumous habits, and with such, enlarged glands are usual, whether they suffer from any such affection or not. Strumous ophthalmia is very frequently attended by enlarged glands, and disappears as the enlargement subsides, yet it furnishes no argument of the dependance of the one on the other, both being the result of a common taint in the system. Enlarged glands are indeed, apt to appear with most strumous diseases of infants. Mr. Swan states that he has frequently found the glands within the chest enlarged in children who died of hydrocephalus, yet I question whether there was any such phenomenon as crowing in a single one of these cases.

Mr. Robertson very fairly inquires, "is it probable, that in a soft yielding structure like the throat, absorbent glands should, by pressure on the par vagum and recurrent nerves, cause a diminution or extinction of nervous energy?" If the pressure could be supposed sufficient, no one could doubt the effect: the question for consideration is, can it be so? The only satisfactory instances of such effects from such pressure, adduced by Dr. Ley, are those cited from Sir Charles Bell's work, in which partial paralysis of the face occurred from the pressure of an enlarged gland on the seventh nerve between the mastoid process and angle of the jaw. Even in these, however, the analogy is not perfect, as one may readily conceive, that an equal degree of pressure would occasion far more important effects in that situation, than in the lower and less resisting parts of the throat. If the pressure of enlarged glands could in the latter situations so readily affect the energy of the nerves, it is wholly inconceivable that it should not more frequently happen in strumous children with enlarged glands; and admitting for a moment, the assumption that the effects of the pressure would be immediate, it seems extraordinary that it should not often take place in those violent strainings or twistings of the neck, which in their games and exercises so continually occur. This last observation, I have made, because Dr. Ley states, in explaining the

occasional absence of the crowing, "that glands not instantly in contact with a nerve may, during the varied contractions of the muscles of the neck in crying, coughing, laughing, or sudden twisting, produce at once contact and compression, *when temporary asphyxia will be the result.*" I do not, however, believe, that momentary pressure on a nerve, however great, occasions any inconvenience at all, of which we have every day evidence in our own persons. To impair the energy of a nerve, the pressure must be continued for some time, and when once impaired, it is a considerable time before it is again recovered, although the pressure be removed.

This brings us to the consideration of the paroxysmal nature of the disease, which Dr. Ley acknowledges he has always looked upon as the strongest argument for its spasmodic character. Adopting Mr. Swan's explanation of the paroxysmal nature of all convulsive and painful disorders of nerves, quoted by Dr. Ley, "a nerve cannot at first bear a diseased action without rest, any more than a healthy one, and therefore the diseased action after a certain period, ceases to make any impression; but after this rest, the nerve acquires fresh powers, and is again fitted for the same action. In palsy, on the other hand, the pressure being permanent, the loss of sense or motion, or of both, is permanent also." If this were universally true, it would be altogether fatal to Dr. Ley's supposition of the paralytic nature of laryngismus stridulus, as he would find it impossible to reconcile the rapid recovery, easy respiration, and long intervals of perfect relief between fits occurring on the same day, with permanent defect of power. But it is only true, he says, as far as the nerves of sensation and volition are concerned; for in paralytic ailments of the muscles supplied by respiratory nerves, the palsy, though continuous, is not constantly manifest; and in proof of this, he instances the effect of pressure on the portio dura, the defective power occasioned by which is not observed, till in speaking or in emotion, or difficulty of respiration, vigorous action of the muscles is required.

So in like manner he says, in the glottis "the effects of the enfeebled, if not paralyzed, state of its opening muscles, are only observable in those more vigorous efforts which are made when the respiration is hurried or impeded, as in fright, fits of anger, sudden awaking from sleep, in consequence of some external impression, and in screaming, crying, coughing &c."

It appears to me, that the whole question of the spasmodic or paralytic nature of the disease turns chiefly upon the correctness of this distinction which Dr. Ley has drawn between the voluntary and respiratory nerves. To take the instance which he has himself put forward in illustration, that of the seventh nerve, he has certainly fallen into error, probably in consequence of having seen no case in which it was perfectly palsied. In perfect or even in very considerable palsy of the portio dura, I have always seen the face permanently dragged to one side, whether the muscles were in active contraction or not. In slighter cases, where the energy of the nerve is not much impaired, I am aware the defective power is not observable until the opposing muscles are thrown into vigorous action, but this would be equally true, if the opposing muscles were supplied by purely voluntary nerves. The law that if the pressure is permanent, the defect of power must be permanent exactly in the same degree, is in fact true of the portio dura, as it is of all other nerves; and referring this law to the glottis in paralysis of the recurrens from pressure, whatever effect is produced, and in whatever degree, must be as permanent as the pressure that occasioned it. I do not here take into account the greater defect of power observable in the palsied muscles of one side of the face, when the opposing ones are in vigorous action, or by those of the eyelid when we attempt to close it, or in the glottis when the closing muscles are spasmodically or energetically contracted. In these instances the effect arises from active contraction of the healthy muscles, not solely from the palsy of the others, and therefore can last only as long as muscular action usually lasts—a few moments. But even this effect,

in its constant recurrence, if not in its continuousness, bears a permanent relation to the paralytic affection, since no action of the unaffected muscles can take place without its appearing. It is clear, therefore, that if we are to explain the paroxysmal nature of the crowing disease, by any such effects of loss of power in the recurrent nerves, we must assume that when once the paralysis is more or less imperfectly established, whatever ill result or difficulty of breathing is at any time observable in laughing, crying, screaming, fright, &c., should invariably return with every recurrence of these, and a certain amount of mischief, however slight, should be always distinguishable when at perfect rest; just as the slightest defect of power in the portio dura is observable in the slightly parted eyelid while the patient sleeps. But the fact seems entirely otherwise; children affected with this disease pass days without the slightest appearance of illness, between two fits of suspended or difficult respiration, during which they laugh and cry, and are excited as usual.

The foregoing observations must sufficiently display the error Dr. Ley has fallen into with regard to the effects of palsy of the recurrent nerves in occasioning the phenomena of laryngismus stridulus. On the whole, after all the consideration I have devoted to the complaint, and having, I think, given other ingenious arguments of Dr. Ley their full weight, I feel still disposed to continue a disciple of the older doctrine, and to hold the affection as one of spasm or partial convulsion like cramp, rather than of paralysis. The fact of its being frequently benefited by antispasmodics, with which Dr. Underwood tells us he latterly cured most cases, and by anodynes, as opium, hemlock, cicuta, &c., recommended by all modern writers on the disease, favours this view; the circumstance of the sudden occurrence of the gasping and crowing on washing with cold water, laughing, crying, or agitation of mind, also supports it as well as the almost universal coexistence of the carpo-pedal contractions, and the frequent termination of the

complaint in convulsions. But above all these, as a strong analogical evidence for its spasmodic character, I place its paroxysmal nature, and the manner in which the paroxysms occur. The office of the superior laryngeal nerves would lead us to expect a disposition to spasmodic action on the least irritation or excitement, recurring at irregular intervals, dependent of course on the return of the irritation or excitement, but far more on the increase or decrease of the susceptibility of the parts, and disposition to spasmodic action. Dr. Mayo very justly remarks, that "a frequent disorder in parts endowed with acute sensibility, like the mucous surface of the larynx or eyelids, is an increased susceptibility of the sentient surface, and a tendency to spasmodic action in the adjacent muscles, which usually act from impressions received upon it. Thus in the urethra a morbidly sensible state of a part of the mucous membrane produces spasmodic stricture, or a continued contraction of the surrounding fibres of the accelerator urinæ. In like manner is produced spasm of the glottis, in cases of ulcer within the larynx and in hydrophobia." The muscles supplied by the superior laryngeal nerves are the sentinels of the chink of the glottis, as the orbicularis is of the chink or opening of the eyelids, and their action is directed by an exquisite sensibility of the parts, which is at once manifested on the attempted entrance of anything injurious to the lungs, whether it be a drop of water, or a volume of mephitic gas. One can well understand how dangerous any morbid increase of the sensibility of such nerves at their extremities, or any existence of irritation at their origin, might prove, why the danger should occur in irregular paroxysms, and why the exciting cause which occasioned them on one day should be altogether powerless on the next. If it be inquired further, why such a dangerous result as the suspension of respiration in the crowing disease does not then occur more frequently, it can only be replied, that we are wholly ignorant of the morbid condition which disorders the functions of those nerves ; or whether it exists at

their extremities, or their origin in the medulla oblongata, and can therefore offer no reply. If the affection be organic, we should find it more difficult to account for the occasional recoveries under very mild treatment, than the usual fatality under the most active. If it be functional, and therefore symptomatic, we can better understand why it might depend on a variety of causes, at one time upon an affection of the head, at another of the bowels, at another upon dentition; we can comprehend, too, how these several affections, influencing peculiar predispositions, may in one child occasion hydrocephalus, in another convulsions, in a fourth, that more rare infantile disorder, the crowing disease.

Of the treatment of laryngismus stridulus I have but little to offer. In this view of it, which is the one adopted by Dr. Marsh, the obvious preliminary to a judicious plan of cure is to ascertain, by a careful examination of all the functions of the body, whether there be anything wrong to which we could refer the origin of the disease. When any such causes or complications, whether in the head or abdomen, are found to exist, their removal should form our first object, but always with reference to the strength or tone of the system, which in an affection invariably found in connexion with a strumous habit, cannot be lowered much without injury. The maintenance of the general health and strength seems to be on all occasions a matter of importance, but still more so in those cases in which no complication exists, and where probably the complaint very much depends on debility, in connexion with either a cachetic state of the body and wasting, or preternatural irritability with plethora. From my experience of the complaint I am disposed to believe that no treatment, directed entirely with reference to its complications, or on the supposition of its being a pure cerebral affection, can be successful, and I fully agree with those practitioners whose great anxiety is to restore the natural tone and firmness of the system, giving tonics where there is deficiency of power, and antispasmodics and anodynes where there is great irritability. To attain our object,

however, it must always be held in mind that in the chronic affections of children, the exhibition of medicines is secondary to the management of diet and change of air. The former should be regulated with the utmost care, and in many cases Dr. Marsh's suggestion with respect to a succession of good nurses during the whole period of dentition might be attended with advantage. Change of air not only in this complaint but in all obscure chronic cases is invaluable, being in fact the only remedial measure about the beneficial influence of which there can be no question, however our views of the nature or pathology of the disease may alter from time to time. I have often thought that in such complaints, and still more in those which are supposed to be endemic, it would be most desirable, in every instance where it was at all practicable, to treat the patient in an atmosphere differing as much as possible from that in which his disease originated. I believe many cases of croup and cholera, such at least as are not of the rapid type, might be treated successfully if removed into a new atmosphere, which are utterly hopeless when the cure is attempted in the locality where the patients sickened. Whether a child affected with laryngismus have change of air or not, however, it is essential that he shall sleep in a cool and airy apartment, and that when the weather permits he shall be as much as possible in the open air. There is no one point in which children are more mismanaged than in the arrangement of their sleeping apartments, which are commonly the most close and confined in the whole house.

Dr. Merriman has recommended the use of continued purgatives, so as to procure at least two free motions daily. When the child is plethoric, or is otherwise likely to bear evacuations well, it would, I have no doubt, be an advisable plan; indeed in young children the bowels are usually moved as often as this in the twenty-four hours with advantage, but the recommendation, in any case, should be followed with constant regard to the strength of the little patient, and the apparent effect. He

has also advised the use of soda or burned sponge, probably with the same view which induced Dr. Ley to propose the preparations of iodine, to diminish the size of the enlarged glands in the neck and chest, to the pressure of some of which on the recurrent nerves he attributed the disease. Doubtful as I am of the correctness of this opinion, I should think it judicious to adopt the treatment, especially as it could in no sense interfere with other remedial measures on which I should perhaps set more value. It seems to be a fair indication to endeavour as much as possible to counteract or overcome the general strumous disposition in those affections, which are seldom or never found to occur except in conjunction with it.

I have offered these few unsatisfactory observations without wishing to attach more importance to them than they merit. I believe both the pathology and treatment of the disease are still very uncertain, and that it will require all the consideration and inquiry which observant practitioners can bestow upon the subject for many future years to attain a just knowledge of either. The following summary of the amount of our present information and of the facts connected with the disease, may be useful to subsequent inquirers.

By the concurrent testimony of almost all who have noticed the affection, it occurs for the most part, if not wholly, in strumous habits.

It is frequently found in connexion with enlarged glands in the neck, and perhaps in the thorax.

It is frequently found in connexion with eruptions on the face, ears, or scalp.

It frequently terminates in convulsions, and is sometimes, though very rarely, ushered in by them. I believe it may be said that nearly half the fatal cases on record terminated in convulsions.

It is met with in families in which children are subject to head affections or convulsions, but who have also the strumous disposition.

It is sometimes met with in connexion with an apoplectic or comatose state from the commencement, as in the cases of crowing apoplexy which I have described.

In a great proportion of the cases which terminated fatally there was not the least symptom of head affection through their whole course, if we do not look upon the occasional fits of breathlessness and crowing as indicative of it, and the children were as well apparently, a few moments before death, as they were previous to the first attack of the disease, or as any children could be.

The complaint is sometimes, but rarely, attended by cough and permanent difficulty of respiration.

Perhaps it may be said that from one-third to half of all the cases of which we have any account, terminated in death.

ART. XXII.—*Observations on the Use of the Stethoscope in the Practice of Midwifery.* By DAVID C. NAGLE, A. B., M. B., one of the Physicians to the Dublin General Dispensary.

OBSTETRIC auscultation having scarcely as yet advanced beyond its infancy, it becomes the duty of those, who may have had opportunities of appreciating its value, to endeavour zealously to promote its utility and growth. In the several papers which I have published in the *Lancet* on this branch of auscultation I was mainly influenced by feelings of this description; and the same shall guide me, I hope, as well in the present, as in any other paper, that I may hereafter devote to the subject. To every physician, who takes an interest in the science of his profession, and feels a wish to promote and uphold its respectability, it must ever afford a peculiarly intense gratification to add to those records of improvements that are daily being made in this department of knowledge. And if I, who cannot but contemplate with delight the prominent position which my own countrymen are taking in the branch of medical literature

now before us, can contribute anything useful to the general stock, my object indeed will be fully attained.

I shall here briefly recapitulate, what I have endeavoured to establish by those papers that appeared in the *Lancet* since 1830 ; and then submit to the profession a few additional facts that may not be undeserving of attention.

Auscultation affords but two signs of pregnancy : the pulsations of the foetal heart, and a murmur, that should, correctly speaking, be designated the uterine murmur.*

The first is absolutely unequivocal ; the second equivocal ; but, perhaps, the least so of those, that should be considered as such.

The pulsations of the foetal heart may be detected, at all events, between the fourth and fifth month, and vary from 120 to 180 in the minute ; though I have found them, and that but momentarily, to sink as low as fifty or sixty.† They remarkably resemble the ticking of a common watch, and may be detected in various parts of the abdomen, but generally in the iliac regions, particularly during the advanced periods of pregnancy.

They are, though occasionally nearly masked by the uterine murmur in the parts, where that is most intense, easily distinguished by an experienced ear, especially if the cylinder be moved a little from the principal site of the murmur ; or when the foetus takes a roll in the womb.

They are sometimes liable to be confounded with the pulsations of the mother's heart ; but the distinction is drawn with facility by attending to the rhythm, and gradually moving the stethoscope towards the region of the parent's heart.‡

In no case will the beatings of a foetal heart, when at all energetic, escape detection, if the examination be properly conducted, and the ear of the auscultator be familiar with the

* See *Lancet*, December, 18, 1830, p. 396, col. 2.

† *Ib.* November 13, 1830, p. 233.

‡ *Ib.* January 8, 1831, p. 501.

rhythm and peculiarity of sound. But this familiarity is absolutely requisite, otherwise the examiner will be left in a state of perplexing uncertainty, where a practised ear would scarcely experience the slightest embarrassment.

By an intimate acquaintance with the nature of this sound we can readily ascertain the life or death of the foetus in the womb; from which knowledge, as I have already shewn, we can derive vastly important practical benefits. We shall also experience no great difficulty in detecting the existence of twins; and, what I have myself found of the utmost, the most gratifying, practical advantage, we can, in most cases, easily discover the nature of the presentation.*

The other phenomenon, the uterine murmur, I have shewn to be of considerable importance to the scientific practitioner in midwifery. It is the first of the two stethoscopic signs that occurs; and may, in vigorous constitutions, be detected about the third month.† It is not in the least affected by the life or death of the foetus in the womb; and is quite independent of the foetal circulation. Its *principal* site is invariably in the lateral regions, along the course of the lateral uterine arteries; commencing, apparently, opposite the space between the anterior spinous processes, and closely to the iliac arteries.‡

It should not be sought for in the anterior parts of the abdomen; as its occurrence there never happens, unless some branches of the lateral uterine arteries should extend across in an unusually enlarged state. This we can easily verify by moving the stethoscope gradually from the median line towards the ilium; for thus we shall find the murmur to increase in intensity, as we approach the latter.

It is frequently detected in both iliac regions at the same

* See *Lancet*, December, 24th, 1831, p. 449, *et seq.*

† I beg to be considered as offering no decided opinion on this point. I think it might be discovered even earlier than the third month; but as I am endeavouring to avail myself of every opportunity for determining the question, I may hereafter be enabled to speak with more confidence on this subject.

‡ See *Lancet*, January 8th, 1831, p. 497. *et seq.*

time,* though usually louder on one side than the other ; and by repeating our examination, we shall often hear it on the side, where, in our previous inquiry, we could not, perhaps, discover a vestige of it. To this fact I would beg leave to invite the attention of those, who would persuade us that the placenta must be the seat of this important phenomenon. And now let us dwell a little on this controverted point.

All must agree in the truth of the following propositions : 1st. Experience shews that the placenta is liable to be attached to the uterus anteriorly, or posteriorly ; laterally, superiorly, or over the os uteri. Its place of implantation, therefore, is variable. 2nd. The *principal* site—and this is the important point to be attended to—of the uterine murmur can never be detected, except in the lateral regions. Its situation, consequently, is invariable. If, then, the seat of the murmur be in *that* part of the uterus to which the placenta is attached, it should vary with the varying situation of the placenta. But it never does. Consequently, it is but reasonable to infer, that the murmur is not seated in the placenta. Besides, have we it not—modified, no doubt—where there is no placenta ; as when the uterus is enlarged by disease, and the vessels carrying an unusual quantity of blood to the point of irritation ? This I was once enabled to show satisfactorily by a very interesting case.† It has also been found, that, after the placenta was removed, and the uterus resumed a distended condition, the murmur re-appeared. Where then was its seat ?

Every observant and reflecting physician, who has acquired any experience and accuracy in obstetric auscultation, will readily accord his assent to the correctness of the following statement. It is by no means an uncommon occurrence, to hear the murmur, quite distinctly, immediately opposite the space between the anterior spinous processes ; and yet, after delivery, to find the placenta situated high in the uterus ! It is no argument to say, that the murmur may extend so low

* See *Lancet*, December, 18th, 1830, p. 397.

† *Ibid.* p. 399.

from the placenta, for I have already shewn, that if the murmur, heard so distinctly and at so remote a distance, were the effect of radiation, it should become more clear and energetic, as we approached with the stethoscope to the placenta, the supposed point de depart, or centre of radiation.*

If the author of the work on obstetric auscultation, with whom chiefly I happen to be at issue on this point of doctrine, would be pleased to meet these facts and arguments in a manner satisfactory to the profession, I will be happy not only to give up my own opinions, but even to excuse his, in my mind, unsuccessful attempt to throw discredit on the "*suggested*!" utility of being enabled to discover with the stethoscope the nature of the presentation, and the existence of twins in the womb; to which advantages, he will admit, I had directed the attention of the profession long before his work made its appearance.†

During the time I had been engaged in the treatment of cholera in the Townsend-street Cholera Hospital, and whilst superintending that in Kilkenny, I had frequent opportunities of observing the effects of that frightful disease on the foetus in the womb. I felt it fully a part of my duty not to be deterred from attending to this important feature in that awful visitation; for in a Christian land, it is not unimportant to ascertain whether the life of the infant in the womb, notwithstanding the death of the mother, may not be still inextinct in a malady, which, in many cases, ran a course so rapidly fatal. The result of *my* experience is the following.

In every case of profound and perfect collapse, the life of the foetus in the womb was destroyed long before the death of the mother. This, by minute investigation before and immediately after the death of the parent, I satisfied myself of in numerous cases, as those who saw me make the examinations

* See *Lancet*, December, 1830, p. 397, *et seq.*

† See *Lancet*, January 8th, 1831, p. 501; December 24th, 1831, p. 449, *et seq.*

are well aware. Long before the stage of collapse was complete, the uterine murmur ceased to be heard: but the life of the fœtus has not been destroyed by the disease, when that was arrested before the collapse made any great progress. This was satisfactorily demonstrated, not only by stethoscopic investigation, but by many cases of premature confinement that occurred during that period. When the lives of the mothers were preserved by arresting the disease in its early stages, it was gratifying to have been enabled to assure them that their infants were still alive.

Reflection will point out many ways, in which the knowledge, I am endeavouring to inculcate, might be made most usefully available. The following may be considered as no bad instance of it. An intelligent friend of mine was consulted by a female, not long in town, and then appearing to be affected—as she would fain persuade people to think—with some symptoms of dropsy. He suspected the nature of the case, but could get no vaginal examination. He wished me to examine this person, and had a friend of his present, who, I believe, had seen the patient before, but did not consider it a case of pregnancy. We met. It was with difficulty we prevailed on the patient to allow an examination with the stethoscope even. I instantly heard the pulsations of a fœtal heart, and unhesitatingly pronounced it a case of pregnancy. One of the physicians, though long in practice, was sceptical: I wished to remove his uncertainty by getting him to apply his ear to the cylinder. The pulsations of the fœtal heart were very energetic, and about 160 in a minute; whilst those of the mother were remarkably slow, not more than fifty. He could derive no information from the examination, as he was *then* unacquainted with the use of the instrument. No persuasion could induce the woman to admit the possibility of her being pregnant: but the extreme repugnance, with which she permitted even the most delicate stethoscopic examination, would be sufficient to create a suspicion that she was not conscious of her own innocence. I requested of my friend to inform her, that I, at least,

had not the slightest doubt of her pregnancy ; and I even predicted, what actually took place, that she would in a very few days retire into the country for the better concealment of her real state.

To an inexperienced practitioner this case may be instructive ; and I have given it in the hope, that it may contribute to induce those, who have not practised auscultation, to familiarize themselves with the use of the stethoscope, by which only certainty of diagnosis could be obtained in such a case. The following is another striking illustration of what I would inculcat

A female presented herself at the Dublin General Dispensary : her appearance would indicate a dropsical affection. After a few questions I told her that I suspected she was pregnant. She peremptorily denied its possibility. I sought for, and readily detected, the uterine murmur ; and again, with more confidence, repeated my suspicions, but she still persevered in her denials. To remove all doubt from my mind, I examined for, and easily discovered, the pulsations of a foetal heart, and told the patient that she was positively in the state I represented. This I insisted on with so much confidence, that her obstinacy at last gave way ; and bursting into a flood of tears, she admitted the possibility of all I said being perfectly true.

The practitioner in midwifery may infer from the case, with which I shall conclude this paper, what important service might be derived from an accurate acquaintance with the use of the stethoscope in that critical department of our profession. We are well aware that infants often appear to be still-born, and are laid by as such, though the labour seemed to proceed quite favourably, and the attendant physician had most carefully discharged his duty. This calamitous result may be occasioned by the pressure exerted on the head at the moment of its escape ; or by other circumstances over which the physician could have no control. Auscultation, judiciously employed, would have shewn that, in many such cases, the foetus had probably enjoyed vigorous existence a few moments previous to its

birth ; and thus the physician be encouraged to persevere in the use of efficient means for restoring suspended animation. How many valuable lives may be thus preserved ! how gratifying to the conscientious physician must success in his professional exertions, under such circumstances, always be ! and how simple are the means for the attainment of such encouraging information, may be thus satisfactorily illustrated !

On the 4th of October, 1835, I attended, in her confinement, Mrs. Cooney from Trinity College. The labour proceeded favourably, and the presentation was quite natural. The action of the foetal heart, examined frequently, as is my custom, was ascertained to be perfectly regular, and retaining its energy undiminished ; yet the child, when born, seemed absolutely lifeless ; and all appearances, combined with an intolerable foetor of the liquor amnii, which passed off as the head was emerging, were well calculated to leave on one's mind an impression, that the infant was dead for some time. Without the benefit of auscultation I should have been myself under a similar impression ; but from the regularity and strength of the heart's action about ten minutes before delivery, when I last explored it, I concluded that the vital spark could scarcely have been quite extinct. No benefit appearing likely to accrue from allowing the cord to remain any longer undivided ; and having rapidly provided for the mother's safety, I succeeded, after nearly an hour's unremitting, because not hopeless, perseverance, in restoring the infant to vigorous animation.*

* I said that the pulsations of the foetal heart can be detected, *at all events*, between the fourth and fifth month. In proof of this I am now enabled to state, that on the sixth of last October I heard them so distinctly in a female, who had conceived on the 18th of May, that I am convinced I could have discovered them long before. They were much slower than at a more advanced period, but perfectly regular, and so very distinct that I could easily recognize them, though the mother was standing when I made the examination. She mentioned that she felt the movements of the foetus during the previous fortnight. This case will also enable me, I expect, to give an opinion, with some confidence, on another controverted point—the period between conception and the commencement of labour. I have already strong, but not decisive, proof, that that period is 280 days.

ART. XXIII.—*Die Physiologie des Hüftgelenkes in ihrer Beziehung zur Lehre von der Coxarthrocace*. Von Dr. B. GADECHENS, pract. Ärzte in Hamburg.

The Physiology of the Hip Joint, considered in Relation to the Doctrines of Hip Joint Disease. By Dr. B. GADECHENS, of Hamburg.

[Translated from the *Zeitschrift für die Gesamte Medicin*. By S. LENOX L. BIGGER, M.B., L.R.C.S.I.]

No very deep knowledge of the doctrines hitherto entertained with regard to diseases of the hip joint is requisite, to convince that they are far from having arrived at the grade necessary to an accurate science; although much has been done to elucidate them, both in our own and in former times, by the most distinguished practitioners, still they are capable of being shaken in their fundamental principles by every severe attack.

The systematic representations of Rust had deservedly obtained the approbation of the medical profession on account of their exhibiting the phenomenon, how under a deceitful exterior, internal disease might be concealed.

English surgeons also (Falconer, Crowther, and Brodie, had with their usual practical sharp-sightedness, got rid of many diagnostic difficulties, and by their pathologico-anatomical examinations, explained more clearly the seat of the lesion and the morbid products during the progress of the disease. Still much remained to be cleared up and unriddled by Fricke's laudable researches, to which our gratitude is due, because they have freed those dogmas from the fetters of authority, and have first excited serious doubts on the correctness of former views and representations.

We cannot wonder that Fricke did not succeed in banishing all obscurity from this his self-chosen path, or in reconciling what is contradictory in his observations to the observations of

his predecessors, since we learned the valuable discovery of Edward Weber, of the hitherto totally disregarded effect of atmospheric pressure on the hip joint. In vain might the surgeon exhaust his talents in attempts at explanation, so long as a physiological problem of such consequence was withheld from his eyes. But by this discovery the way is opened, and the path indicated, by which we may enter on a territory from which we were hitherto partially excluded. Time will shew, if we shall make a proper use of the clue which Physiology has presented to our hands, from the cornucopia of her newest discoveries.

For this reason we have been induced to give a fragmentary description of the influence which the discovery of Weber has had on some striking points of the pathology of the hip joint. With justice may it be said, that with the discovery of Weber, a new and fortunate era commenced for the doctrines of hip joint disease. Weber proved by simple and convincing experiments, that it was not the muscles and ligaments, as was formerly supposed, which preserved the head of the femur in contact with the acetabulum, but that the acetabulum being closed by the head of the bone in an air tight manner, the femur was held suspended by the atmospheric pressure, just in the same manner as the sucker is in a syringe, the anterior opening of which is closed by the finger. Hence all the muscles and ligaments about the joint, even the capsular ligament, may be cut through, without the weight of the leg causing the head of the femur to recede in the slightest degree from the acetabulum; whilst on the contrary, when all the ligaments and muscles are left entire, the head of the bone will sink from three to four lines out of the acetabulum, if the atmosphere pressure be permitted to act on the upper surface of the head of the femur, by boring a hole from the inside of the pelvis into the joint. Experiments *in vacuo** have further proved this fact, and have put to flight every doubt which could be entertained.

* Müller's Handbuch der Physiologie, vol. ii. p. 124.

Weber himself has remarked, that his discovery might be made use of for the explanation of one particular symptom of hip joint disease, viz. the elongation of the affected limb. He meant that form of idiopathic hip joint disease, in which, in persons otherwise healthy, without any external cause, the thigh bone sinks out of its socket, by which, at first, a remarkable elongation of the diseased limb occurs, and evident limping, (as in Fricke's *Coxalgia, sensu strictiori*,) this elongation is produced in this manner, either through the action of the secreting vessels depositing a watery fluid or a firm substance in the cavity of the joint, which causes that the femur should be depressed from the joint in the proportion of its increase, and that it should sink outwards by its own weight, without the ligaments being capable of giving the slightest opposition.

That this pathological deduction, volunteered by Weber, is not very striking, Lauer* has evinced, and has well remarked that in such process the thigh bone by no means sinks out of the acetabulum by its own weight, but is pushed out of it by a process altogether peculiar; for a watery fluid or a firm substance exuded into the joint, would act quite differently from air permitted to enter through a hole in the acetabulum. The latter is in connexion with the whole atmosphere, and presses the head of the femur with the same force outwards, as it did before the hole was bored, in the contrary direction: but in the exudation this is not the case: here the operation of the atmospheric pressure was only modified and not removed. Other objections may be made to Weber's conclusions. According to his own work, the acetabulum with its ligamento-cartilaginous border, describes only in one direction, viz. from before backwards, a perfect half circle of 180 degrees, but in every other direction a smaller segment. The edge of the acetabulum is placed at farthest only on the greatest circumference of the spherical head of the femur, and as it does not project out

* See second vol. of *Zeitchrift für die gesammte Medicin.*

over this, it cannot at all assist in retaining the head of the bone in the socket. Every force therefore, which expels the head of the femur from the acetabulum, must, since it removes the contact of the edge of the acetabulum, from the greatest circumference of the head of the femur, bring it always to a smaller segment of the egress of the acetabulum. Also in proportion as the head of the femur is forced out, there exists between it and the border of the acetabulum, a greater or less empty space of a circular form, into which the greater part of any watery fluid in the joint must flow of necessity, whereupon also the pressure of the atmosphere, without any activity of the muscles, must instantaneously drive back the head of the femur into the bottom of the acetabulum, consequently the capsular membrane must be perfectly full of watery exudation, before in this manner a permanent elongation of the limb can occur. It follows from this explanation, that Weber must have entirely disregarded the share which must be allowed to the activity of the muscles, as those who are affected with this slight degree of suffering in the hip, and who are accustomed to go about for a long time, find in consequence, the femuro-pelvic muscles constantly in a state of powerful contraction. We can imagine thus, that by the atmospheric pressure acting at the same time, a very small proportion of muscular contraction would suffice to draw back the head of the femur into the acetabulum, and we can also think, on the same grounds, that it could only be by nearly perfect filling of the capsule of the joint with exuded moisture, that any lasting elongation of the limb could occur.

If we cannot unconditionally concur in Weber's pathological conclusions, this however cannot do the slightest prejudice to the weighty influence of his discovery in general upon pathology. For, first, from his experiments it is undeniably proved, that when there is no affection of the joint, and when there is only disease of the muscles or ligaments, be it of what kind it may, by no means can any separation of the head of the femur from the acetabulum, and a consequent elongation of the leg, be

effected. Even perfect paralysis of the muscles cannot produce it, a fact which any one can convince himself of, if he should still hold such proof necessary.

The evidence which Fricke has given on the same disease, to which he has applied the name of *Coxalgia Sensu Proprio*, is no longer of any avail in the present state of knowledge, but must be replaced by other proper to the times. The experiments of Weber with the above mentioned limitations offer themselves as simple and uncontradicted, and one might in the greater number of cases of *coxalgia*, in the sense in which Fricke applies that term, form a tolerably correct diagnosis, when he felt disposed to account for the lengthening of the limb, either by a serous or purulent effusion, or even by an extravasation of blood into the joint. Collections of this kind are also formed by the abundant communication between the *bursa iliaca* and the joint, and by the common occurrence of dropsy in the bursa; by the transference of the same to the hip joint, the occurrence of dropsy, or of very marked symptoms of disease of the joint, is more easily discovered. Two arguments appeared to be opposed to the theory of Weber: viz. 1st, the pretended results of experiments instituted by Fricke on subjects: and 2nd, the constantly remarked wasting of the muscles of the thigh observed by him in cases of *coxalgia sensu strictiori*, which will not permit us to exclude altogether the sympathy of the muscles. What may be the value of the experiments of Fricke practised on subjects, is not clearly intelligible, which consisted of freeing the head of the femur from its connexion with the joint, by a simple incision, and having wrapped it in linen, so replacing it; or those of Weber in which the head of the bone was not surrounded, but the acetabulum filled with charpie, for in measuring the limbs in both of those cases, no difference could be found. For as it is proved that by the simple access of air to the joint, the head of the femur is separated three to four lines from the bottom of the acetabulum, and the whole leg is even so much elongated, so we cannot

conceive but that if in these experiments the same space which was filled with air were filled by a firm body, that there would not be an equally remarkable elongation discoverable by measurement. The difference appears to arise from this, that the experiments of Weber were made on bodies, the inferior parts of which were hanging down; whilst those of Fricke were made on bodies placed in a horizontal position. In the latter cases the limb could not be acted on by its own weight, but on the contrary, being brought into the same horizontal position with the other, whilst the artificially enlarged head of the femur which it would be now impossible to return into the acetabulum, would occupy an unaltered situation, probably external and superior against the edge of the acetabulum; a circumstance which would make these different results of measurement easily intelligible. The wasting of the muscles, however, which Fricke gives as the constant and characteristic symptom of his coxalgia, and sometimes a peculiar softness of the muscles to the touch, contains nothing dissonant to the etiology of Weber's hip joint disease; however, in place of a primary symptom, as declared by Fricke, it should be taken as a secondary one, as the patient is compelled to support the body on the sound limb habitually, and only to draw the affected one, which does not offer him proper security, after him; on which account its muscles from inaction, necessarily degenerate into a state of softness, which is much increased when a quantity of fluid is collected in the joint, and perhaps aggravated by the unaccustomed distention which the muscles suffer. *En passant*, we may remark, that this peculiar and equally doughy softness of the muscles, may find its explanation in a sentence of Weber's, which may here be introduced. In his mechanism of the joints, he says: "The condition of the muscles during life alternates between the extreme of stiffness and the extreme of softness, much more than after death. Also after death the difference between the muscles during the rigidity which succeeds it, and after that is passed, is very great: during life, on

the one hand, the muscles can, during a violent effort, attain, for a short period, a stiffness and hardness far greater than that occurring after death, and which makes them like to the bones : on the other hand, their softness, when perfectly relaxed during life, exceeds in a similar ratio their softness after the stiffness of death has passed.

It may further be inquired ; if an elongation of the leg never can arise through simple atony or paralysis of the muscles, without there being at the same time an affection of the joint, how on the contrary can it have any connexion with the shortening of the limb ? Can this be effected by simple sympathy of the muscles, or by a condition of permanent contraction, or by spasm of the muscles ?

So long as the operation of the atmospheric pressure upon the hip joint was unknown, and the generally received opinion was, that the femur was held suspended in the acetabulum by the muscles and ligaments, and that the head of the femur possessed a certain space for motion, which was decreased by every action of the muscles, and increased by every relaxation ; there was nothing unnatural in the theory that a simple condition of irritation of the muscles might produce a permanent shortening of the leg, and we need not wonder that the muscles were supposed to play the first parts in the shortening occurring in hip joint disease, as well as in contusion. Weber's researches have also instructed us better on this point, for if the head of the femur be in a condition to close the acetabulum in an air tight manner, then there can remain no unfilled space between the articulating surfaces of both, as each must move on every part of the other, for we cannot take into account the perfectly unmeasurable space which the synovia occupies. That this was really the case, Weber convinced himself by making several sections through the whole joint in various directions, and also by taking casts of the head of the femur, and of the acetabulum in plaster of Paris. The solid half sphere, taken as a cast from the head of the femur, fitted so exactly in

the hollow impression from the acetabulum, when the unevenness arising from the cavity for the ligamentum teres had been taken away, that a small slip of paper prevented in a remarkable manner the entrance of the artificial head into it, which would not be the case if both surfaces of the joint were not segments of equally large spheres.

Hence we can no longer speak of space existing between the head of the bone and the acetabulum, which, on Palletta's authority, was regarded as proved, and shortening by the mere action of the muscles could only be conceivable by a consideration of the practicability of the cartilages covering their surfaces being compressed, as Fricke, Wernher,* and others have thought to be the case. Yet if any one would consider that the thickness of those in most places is only one line, in a few a line and a half, and at the most three lines, for both cartilages considered together, and if he should try to compress pieces of cartilage which were cut off between two masses of wood or glass, and found the remarkable resistance which they give to great pressure, then would he be very little inclined to lay great stress to attributing the shortening to this cause, and particularly if he had experienced how uncertain measurements are in living persons, when it concerns so small a space as one line, or half a line. Wherefore, the muscles have just as much power by their contraction alone, to produce a shortening of the limb, as on the contrary, by mere relaxation to produce an elongation. But even in organic injuries of the joint, in real hip joint disease, it will depend entirely on the situation and the circumference of the lesion, whether shortening may follow it or not. We have seen that the head of the femur and the acetabulum are closely attached to one another, like an hemisphere closely and accurately embraced by a capsule. Such an hemisphere can only sink deeper into its capsule by a diminution of its whole volume, or the capsule must be enlarged in

* See Schmidt's *Jahrbücher*, vol. xii. Part 1, p. 108.

every part. Any partial alteration in the relative dimensions of both, would only remove in some situations the motions of one surface on the other, but would not cause the entire convex surface to pass deeper into the concave. Thence it would be possible to remove a large segment of a sphere from the head of the femur, as in Fricke's fifth experiment, or a similar portion from the back part of the acetabulum, without causing by this means any shortening, and consequently the destructive caries in hip joint disease must have already included the entire superficies of the head of the femur, or of the acetabulum, before evident shortening can be found as the effects of it. This agrees perfectly with the experience of Rust, and of many others, who only found shortening in rare cases, and such as might be considered more properly as exceptions to the first stage of hip joint disease; yet they regard it as a constant and necessary symptom of the earlier stages of the disease, and that it never is absent where the diseased limb, on a superficial examination, appears longer than the sound one: thus any one might imagine at a first glance these two declarations are so evidently opposed, that if one of them stand, the other must necessarily fall. Both, however, can be associated together, without difficulty, as we shall presently shew; and we have only to discriminate accurately the observations from which these two opinions have been deduced, and avoid transferring the characters of one set to the other. It is well known that English surgeons (Falconer, Crowther, and Brodie) first drew attention to the remarkable protrusion or sinking of the pelvis of the affected side in hip joint disease, and distinguished from real, an apparent elongation of the limb, arising from this oblique position of the pelvis. Brodie endeavoured to determine the real, by measuring the distance between the anterior superior spine of the ilium and the patella, which measurement Fricke modified in a most useful manner by selecting in place of the patella, the external inferior condyle of the fibula, which afforded a less moveable and more equable point

for placing the measuring instrument upon. By this improved method, and by the most careful examination of each individual case, Fricke obtained results always the same, viz.: that in apparent elongation of the limb, there was always real shortening, which amounted nearly to the same extent as the apparent elongation.

This fact, to that period overlooked, has been placed by Fricke's numerous measurements beyond all doubt, and now all that is requisite, is a correct and physiological explanation of this phenomenon.

Fricke looked upon an increased contraction of the muscles of the hip, to which they are excited by the continual pain in the joint, and by means of which the head of the femur is pressed more powerfully into the acetabulum, as the cause of the shortening which is found. On one side, however, there is opposed to this what we have said above with regard to the connexion of the head of the femur to the acetabulum, and also the slight compressibility of their cartilaginous envelopes, and on the other side, that in all cases of hip joint disease, a permanent state of contraction of the muscles has not been observed; for how would such permanent contraction of the muscles agree with the great depression of the pelvis on the affected side, which we concur with Brodie in considering, to be, without doubt, a passive state, from the usual gait and limping of persons afflicted with morbus coxæ, both of which are attempts to avoid every muscular effort of the affected leg, and to throw the whole weight of the upper part of the body, and every motion demanding exertion on the sound limb. Also the diminished rotundity of the muscular parts about the joint, and the well known state of relaxation of the glutei muscles, are not favourable to this opinion; finally, it is unlikely, that by perfectly different degrees of intensity of inflammation and of pain, at different periods, and in different subjects, that the muscles should ever find themselves in an over-excited condition. Besides the feeling of contraction in the muscles, to the hand of the examiner, can be easily explained, for at the very moment

of the examination, the pain which is caused or increased may give rise to an instantaneous contraction of the muscles, through recurrent branches of the inflamed nerves when touched ; this, however, proves nothing with regard to a permanent and pre-existing state of contraction.

All these grounds are more particularly supported by this, that every phenomenon of real shortening with apparent elongation, explains itself most perfectly in another way, by which we can abstract the greatest part from the agency of the muscles. Fricke has already drawn attention this, that the appearance can be produced just as well in persons in health, and that any one can remark it easily in his own person by holding both legs together, whilst he endeavours at the same time to make one leg longer than the other, by which act the apparently elongated limb will actually measure three or four lines shorter than the other. " This phenomenon," proceeds Fricke, " has the same cause as in morbus coxæ, inasmuch as by the effort to make one leg longer than the other, all the muscles in the thigh of the former are placed in a state of activity, and by the depression of the pelvis the head of the femur is driven with still greater force into the acetabulum." But it is not alone in healthy living men, but also in subjects, where no consideration of muscular action can enter, that this experiment can be illustrated, with the same results, if one of the lower extremities be fixed whilst we lay hold of the knee and heel, and raise it upwards whilst kept in a perfectly extended state, slowly beside the fixed extremity, during which, however, even the slightest bending of the knee, or any turning of it inwards must be carefully avoided. Then it may be seen that in the same proportion as the crest of the ilium on the shortened side is raised higher, the opposite one sinks down towards the fixed limb, and gives the same result on measurement of the elongated side from the anterior superior spine of the ilium, to the external condyle of the fibula, as it does in healthy living persons, viz. a shortening of from three to four lines in comparison

with the length of the leg in the ordinary position of the pelvis. That there is no forcing of the head of the femur against the bottom of the acetabulum in this experiment, but only a simple lateral inclining of the pelvis around the head of the femur of the fixed leg, stands in need of no proof. If any one, however, was determined to retain the idea of violent pressure into the hollow of the acetabulum causing shortening, then such a person would find it sooner in this experiment on the apparently shortened side, as here the obliquity of the pelvis is caused by the powerful shoving upwards of the whole extremity against the pelvis. Here, however, not only is there no shortening of the space between the spine of the ilium and the external condyle of the fibula, but on the contrary an elongation, and this condition, which was over-looked by Fricke, gives us the best explanation of the whole phenomenon. It is thus that this happens, by every inclination of the pelvis, the whole ilium of the depressed side comes nearer to the trochanter major, whilst on the opposite, the ilium recedes in the same degree from the trochanter major of the other leg, by which circumstance an elongation occurs here, and a shortening there, of the space between the trochanter and the spine, and consequently the same must occur in the whole limb. This process may be rendered more evident by considering the head of the femur as the centre, and the leg itself as the radius of a circle, the periphery of which marks the distance of elongation which would be described by a lateral rotation of the heel in the horizontal position of the body. The anterior spine or any other point of the half pelvis would stand inside this circle near the centre, and thence, according as the leg was moved to one, or the other side of the circle, at one time it would approximate to the under side of the leg, at another distance itself from it, by which means the space in the first case would be shortened between both points, in the latter increased. Now what can be effected by lateral motion of the leg in its socket, whilst the pelvis is fixed, can also be accomplished whilst the legs are

fixed by lateral twisting of the pelvis on the thigh bones, although on account of the circumscribed motion not to the same extent. Hence it happens that the real shortening which is always found with apparent elongation of the leg, and at the same time on the opposite side, the real elongation with apparent shortening, alters only so far by an action of the muscles, as can be produced by inclining the pelvis obliquely to one side or the other by the action of the muscles during life; but the principal cause of this appearance is to be found in each lateral motion of the pelvis, or in other words, in the altered position of the portions of the bone of the pelvis to those of the inferior extremities.

That this fact, which has hitherto been disregarded in an extraordinary manner, cannot remain without influence upon the formation of doctrines of hip joint disease in general may be readily imagined. The first and proximate argument which we would deduce from this is, that in all cases where already in the first stage of *morbis coxæ*, a depression of the pelvis occurs on the suffering side, also a real shortening of the affected leg is to be found, and that thence the assertions of Rust and Fricke, so contradictory in appearance, are only to be distinguished into those cases in which obliquity of the pelvis has not appeared, and those in which it has, in order that they may coincide without false conclusions with one another. The distinguishing of those different cases is of so much the more consequence, as it appears from Fricke's observations and accurate measurements, that the obliquity of the pelvis in *morbis coxæ* is much more frequent than Rust and other German surgeons are disposed to consider it, and that it is not to be regarded as a rare exception. Perhaps Fricke saw it abundantly because his examinations of this condition were principally made in hospital practice, and also almost entirely on persons of the labouring class, who, when the inflammation did not come on very severely, which it is well known is seldom the case, pursued, as is invariably the case, their ordinary avocations, after

the access of the first symptoms had passed, and engaged in laborious occupations, by means of which the promotion of a depression of the pelvis on the diseased side must be necessarily favoured. If on the contrary the patient can from the commencement keep his bed, and observe a quiet, horizontal position, the pelvis can hardly be altered in position, to which Brodie has already drawn attention: "that the apparent elongation of the extremity, is very soon lost by the patient being kept in a perfectly passive condition."

But it is not alone in the theory of morbus coxæ, but also in that of contusion, that it is of importance to know that a simple change in the position of the pelvis with regard to the inferior extremities, or of those to the pelvis, is sufficient to produce a so called real shortening of the extremity. Contusions occur to the thigh principally on the outer or posterior side, and shortening of the extremity, whether apparent or real, which appears hitherto not to have met with due consideration, is a very frequently accompanying symptom. The opinion formerly given, and which was regarded as satisfactory, namely, the pressing in of the head of the femur into the acetabulum by increased muscular contraction, can avail as little here as in the first stage of hip joint disease. On the other hand, the discovery of a depression of the pelvis on the injured side, [compare this paper with Guérin, vol. ii. p. 224,] or an external abduction of the leg, without more, is sufficient to unveil the cause of the shortening, and we have only to determine whether the lateral approach of the pelvis and of the inferior extremity be caused by muscular contraction; that is, whether it be active, or, according to the opinion of Brodie in morbus coxæ, a passive sinking down of the affected side; because the male patient bears the whole weight of the body on the sound extremity, in order to diminish the pain, and the female patient takes advantage of it, in order to support the equilibrium of the body.

Sufferings from contusions on the outer side of the leg will often be explained by tumours and abscesses, consequently such

occurrences in the neighbourhood of the joint must not be overlooked. For want of individual experience, I must leave the application of this subject to those who have enjoyed opportunities more abundantly and accurately of proving those points by clinical observation, and which I have only spoken of in a physiological respect.

To induce further practical research, we will only remark, that hitherto every one was deceived when he considered the actual length of the unaffected side, when previous obliquity of the pelvis had existed, as the normal length of both limbs; for as we have shewn, also on this side by the altered position of the pelvis, the ordinary relations of the dimensions is modified, as is exemplified in the apparent shortening of the sound limb, which most frequently is the case, and where constantly real lengthening is to be found. Above all, this limits in a certain degree the value of general measurements; for it never will be easy to calculate the natural length of the extremities before the pelvis became oblique, after that occurrence has taken place, particularly if already, in consequence of the process of inflammation, changes of form may have occurred to the acetabulum or to the head of the bone, which may have their share in the production of real shortening. However, if the measurements be made with circumspection and prescience, it will always be a diagnostic assistance of the greatest consequence, and it might, if pursued after the method indicated by Fricke, possibly, give unerring results.

In conclusion, I must speak my conviction, that after the disclosures, which on the part of physiology we have given on this subject, a perfectly new course of pathological research is required, in order to raise our theories of hip joint disease to the height of a perfect knowledge, of which is now capable, and which is now more than ever necessary. Particularly is there urgent necessity, that at last by accurate diagnostic signs the diseases of the hip joint should be distinguished from those of the parts surrounding it, as well as those diseases of the

joint which are not inflammatory from genuine morbus coxæ—a wide field for surgical inquiry, and in which a rich reward is to be earned.

May these lines, which proceed from the purest motives of affording information, serve at the same time as a challenge to the honoured editor of this periodical anew to turn his particular attention to a portion of pathology which, under far more difficult circumstances, evidently demanded, and always was treated by him with attention not to be mistaken. The valuable opportunity which presents itself of observing diseases of the hip in our General Hospital, (Hamburg,) allows us to hope still more useful results, from a recurrence of observations on diseases of the hip, with regard to the modifications of the physiological foundations, now become necessary; as well as from similar researches in many other clinical institutions, and we may rely on a careful attention being paid to the cases which may occur, for the advancement of science, so much the more certainly, as the surgical section has never shrunk from its engagement, but has always in the most praiseworthy manner promoted the cause of knowledge, and the science of medicine, after a due regard had been paid to the patients intrusted to its charge.

BIBLIOGRAPHIC NOTICES.

Mr. FAUSSETT's *Reply to the Medico-Chirurgical Reviewer, with Observations on a certain Form of Epigastric Chronic Congestion.*

THE editor of the Medico-Chirurgical Review has done me the honour of noticing my paper on "*Violent Pulsations in the Epigastric Region, and their Treatment,*" which appeared in the Dublin Journal of July last. My reviewer is of opinion that I have offended against the laws of brevity: had it not been my *wish* to avoid obscurity, while it was my object fully to develop some views upon the nature and treatment of epigastric pulsations which were not previously entertained, or at least published, I *might* have been more concise. My reviewer, however, "*quarrels*" with me, because I have not walked implicitly in Baillie's footsteps, but dared to describe with truth what I had myself observed with attention, departing from the path of "*other physicians and surgeons who must have had opportunities of verifying the general correctness of his (Baillie's) observations.*" To appear as wise as Baillie, and to get credit for thinking and seeing all that Baillie thought and saw, are, no doubt, high pretensions, and appear to constitute my reviewer's *ne plus ultra* standard of perfection, as well as a pinnacle to which *his* ambition soars. To wander from such high authority looks like *innovation*, it is an affectation of discovery, and savours strongly of arrogance. Did Baillie recognize epigastric pulsations in persons in the middle periods of life, and never at a period under thirty? My reviewer would rather take Baillie's word *for ages*, than the written testimony of a parish register. Should a greater number of females than males present themselves with epigastric pulsations, my reviewer would be ready to declare, that half of them mistook their sex, and were — "*Highlanders with petticoats prolonged,*" because, forsooth! BAILLIE SAYS, "the complaint is more commonly to be found in men than women." "But," quoth

Baillie, "I am unacquainted with any means of *curing* this affection." How will our critic act now? Think you that he has lost his sense of politeness, and will be found differing from the learned? No! he prefers even ignorance *in good company* to knowledge emanating from an humble source, and would, therefore, "*pause* before he applied the remedies advised," because he thinks them "powerful." I admire my reviewer's prudence as well as respect for the memory of Dr. Baillie, and as a junior member of the profession to which I have the honour to belong, I trust, I am not, myself, wanting in a due deference for authority, or in a suitable regard for the opinions of my superiors, yet "*Vallius addictus jurare in verba magistri*;" if I chance to meet with a woman of *twenty*,* the subject of epigastric pulsations, I would not like to insist upon her being *thirty at least*, merely because Baillie fixed that age as a limit of life for the occurrence of the affection; and so with respect to other differences of opinion, where conscience, and the evidence of my senses, led me to dissent from high authority. Again, I would find it hard to admit, that Baillie's not venturing to account for the phenomena of epigastric pulsations, should inculcate a spirit of supineness in us, or put a bar upon our investigations, or that *his* being ignorant of a *cure*, should necessarily entail a like ignorance upon all posterity. But let our critic proceed.

"We suspect that the subjects of the affection which Mr. Fausset describes, have been principally females [not *Highlanders*? quere?] affected with those anomalous hysterical symptoms which ape all maladies."

My learned critic's suspicions are at variance with his own quotations; but let him continue.

"In them too, of course, there is epigastric tenderness; we say of *course*, because such patients have tenderness in any part to which attention is directed."

I beg to assure my learned reviewer, that with such purely hysterical cases my paper has had nothing in common, as he can inform himself *simply* by a reference to its contents.

Within the last few weeks I have met with three cases of epigastric pulsations, two of them occurred in females, but as my reviewer may believe the masculine gender to be less "*hysterical*," as well as otherwise *more worthy* than the feminine, I shall introduce to his notice a male subject.

* Such a case has been referred to in the original paper.

CASE I.—John Cowser, aged twenty-eight, (ought to have been “thirty,” *secundum Baillie*.) the butler of a gentleman living near this city, complained of occasional inordinate pulsations at the epigastrium, with extreme tenderness on pressure there; slight pressure caused much uneasiness, but an increase of it very considerable pain, yet *really*, and “*of course* this patient had” *not* “tenderness in every part to which attention was directed;” I say, *of course*, because the patient *most obviously did not* labour under that anomalous form of hysteria, “which apes all maladies,” but under an unpretending state of visceral congestion, or over-fulness of the abdominal epigastric vessels, implicating, most probably, the ganglionic nerves and plexuses, at all events deranging the functions of several of the organs which those nerves supply. Waving theory however, let facts speak for themselves. With epigastric tenderness, there was fulness, a great sense of vital depression, emaciation, loss of strength, loss of appetite, headach, sallow complexion, pallid lips, torpid bowels, cold feet, &c. &c., pulse undisturbed. Such were the leading symptoms. The following was the treatment. He was twice leeches and then cupped at the epigastrium, after which, antimonial ointment was freely rubbed over this region; his bowels were acted upon by mild aperients, and then, blue pill combined with henbane and ipecacuanha, as in the former cases, was directed to be taken twice daily, until the gums became tender, *not* so as to produce “salivation.” His diet was made to consist of milk and the farinaceous vegetables, animal food during the medicinal treatment was abstained from, and wine and all stimulants were strictly prohibited. I have lately had the gratification to learn, that in about a fortnight from the adoption of the above course of treatment, the patient’s health became almost entirely renovated. There was in this case no indication for general bleeding, and if practised, it would have, undoubtedly, proved injurious. Topical bleedings, however, followed by counter-irritation, were attended with decided benefit.

Whether such a case as the above, in either male or female, should come properly under the denomination of “*hysterical*,” or whether the result would not in *any* or in *every* case, justify the view taken of it, as well as the practice put in force, let the candid reader decide; for it is with *such* cases, and *such only*, that my paper has had any thing to do, and *to such*, that, in any respect, the principles of the treatment applied; all of which my learned, if not *too hasty* reviewer, might have very well conceived from the whole tenor of my descriptions, as well as from having expressly disclaimed any allusions to cases “*merely hysterical*,” or to “*those cases of pure nervous*

debility, demanding the use of tonics, shower baths, and generous diet." How my reviewer, a rigid disciple, "according to his own *shewing*," (for which learned expression see *Johnson's Dictionary*, if not there see *Johnson's Review*.) of Baillie, and an admirer of Cullen, could have seen "*several cases*" such as I have described, and yet mistake them for hysteria, admits of conjecture. The reader may pardon such attempts to delude him, but Bright, and the shades of Cullen, Hamilton, Good, and Armstrong, together with the ashes of Sydenham, are afterwards to be appeased. The two complaints are essentially distinct, as well as easily distinguishable from each other by the exercise of common attention.

In hysteria there is seldom much loss of flesh. In the complaint, of which epigastric pulsations form *but a symptom* (and which, perhaps, ought to be called by the name of *epigastric chronic congestion*), emaciation is generally considerable. Hysteria, properly so called, is almost peculiar to women. Epigastric congestion, if the title be allowed, is also to be met with in men, yet less frequently in men than women. Hysteria may occur at puberty, the time of its occurrence being generally that of the menstrual period. Epigastric congestion has no necessary connexion with the menstrual secretion, has not been described as occurring with such a train of symptoms so early as puberty, and *cæteris paribus*, is oftener to be met with at the middle periods of life than hysteria is. In the one complaint there are alternate fits of laughter and crying. In the other no such symptoms are to be noticed. In *epigastric congestion* there is often epigastric tenderness differing in different cases, but always increased by pressure. In hysteria pains are vaguely, and at random complained of, being often as much aggravated by pointing at the site of pain, as by pressure. Finally, in epigastric congestion there is no hysteric paroxysm, no "*clavus hystericus*," no "*ventris murmura*" of Cullen, no "*sensus globi in abdomine se volentis, ad ventriculum et fauces ascendentis, ibique strangulantis*," no "*sopor, convulsiones, urinæ limpidæ copia profusa, animus, nec sponte, varius et mutabilis*."

The suffering is of a different kind, and the countenance betrays it; there is a steady, earnest, unpretending look of distress, a look of absolute, not fanciful endurance; add to all which that "*hysteria is always relieved, often cured by tonics, and frequently benefited by stimulants*,"* while epigastric congestion may generally be cured by a directly opposite method of treatment, and the blood, which in a severe case, it may be necessary to abstract, will be found both buffed and cupped.

* See Bright's Medical Reports, vol. ii., part 2.

Let not my reviewer then again confound this disease with hysteria. Mason Good, Cullen, and Armstrong, describe the latter affection with some accuracy; I would recommend him the works of those authors. Even a *hasty review* may add *correctness* to some future critique. *But*, our learned censor "has met with cases of this disease, [epigastric congestion,] *as well as his neighbours.*" This classical, and gratifying assurance imports, no doubt, some corresponding advantage to the profession, as well as to mankind at large; but no! our critic's experience, and the results of his treatment, are locked within his own breast—he is silent upon the subject, at least not particularly communicative. "For our own parts," says he, "we endeavour to discover the cause in every case, and having discovered, to remove it." This, indeed, is philosophical! but *has* our reviewer "in every case" discovered the cause? or has he in *any* case removed it? if so, what remedies has he used? are they essentially different from those which have been referred to, (and which of course ought to be used *discriminately*, and modified to the case)? if they are, I will venture to affirm that his remedies have been applied to cases entirely different from those which Baillie describes, and with *such only* we have to do; if they are not, and that he selects cases "whose symptoms ape all maladies," and applies *to them* the treatment in question, *then* his success will be in proportion to his judgment. It is to be hoped, however, that my reviewer's hastiness to decide does not characterize his practice, as it undoubtedly does his criticisms, else his patients and readers will have a common cause of quarrel with him.

But, to continue: my "*theory*" has received an especial mark of my reviewer's displeasure; for he thinks my reasonings "*hypothetical and not conclusive.*" He would, however, do well to remember, that my reasonings were derived from facts, and not from hypotheses, and that such facts were given merely as "*so many evidences* of congestion, or chronic inflammation, implicating the ganglionic nerves and plexuses." But, I have no right to theorize at all, because I have "*discarded speculation.*" I must here remind my reviewer, that while I tried to distinguish what was theoretical and doubtful, from what was practical and known, *I did not* disclaim for myself the right of forming a just theory, to account for the phenomena of epigastric pulsations, and their accompanying symptoms, as well as for the marked and uniform success of a particular method of treatment. *But*, "Mr. Faussett's theory, is not entertained for nothing; it is the foundation of his practice." My reviewer's courtesy is equal to his candour; else, if he were not in haste

to censure, he might otherwise interpret plain language, e. g. "The treatment comes easily to be inferred from such a view of the subject; but, *the fact is, such a view of the subject, imperfect as it is, has, in a large measure, been derived from the results of treatment,*" &c. &c. (*Dublin Journal, July, 1837.*)

Upon the whole, my reviewer is out of humour with me; my "paper is too long," my "theory *inflammatory*, and" my "treatment not to be lightly used." However, the 1st of October was approaching; despatch became necessary; the reviewer's brain was, no doubt, excited; his gastric nerves in a state of sympathetic irritation, and the whole man disquieted; the easterly winds might have had *their* influence. Yet, were there no silent and uncomfortable reminiscences about "several" of those "cases of this affection," which my reviewer had "*seen as well as his neighbours,*" but which, perhaps, his neighbours had *noticed*, and, it may be, treated better than the reviewer. *Mental association* is a quick and wonderful process; I must, therefore, make "*allowances*" for the critic, particularly as he recommends that some should be made for me.

At last, my reviewer will leave me with the reader, and I thank him for the appeal. I would thank him still more cordially, if he had not, by garbling evidence, forestalled judgment. It is said that a blue pill, followed by the black bottle, wrought wonders upon "*the mood hypochondriac,*" in Abernethy's time. If I might venture to prescribe for my reviewer, I would recommend him this remedy; it may dispel the vapours, remove obstructions, sharpen his understanding, and soothe his mind. At present, he is scarcely in a capacity to digest the subject of epigastric chronic congestion. I beg, therefore, to call the reader's attention to the following:—

1st. Epigastric pulsations are *but links* in a morbid chain of sympathies, whose *proximate cause* is in all cases the same; which position follows from two facts: first, that the symptoms differing *in degree*, in different cases, are always of the same class; second, that the same plan of treatment, modified according to the case, is uniformly attended with the same result.

2nd. There is in every case of epigastric pulsations, an overfulness of the abdominal epigastric vessels, either constituting congestion, or attendant on subacute inflammation; if this were not absolutely the case, depletory measures would not be attended with such good success; local bleeding would not be found so specifically useful, the blood abstracted in severe cases would not be found buffed and cupped; the sense of vital depression would not be diminished, but rather increased, and the patient, so far from being strengthened, would be weakened, and have all his ailments aggravated.

3rd. If the symptoms be all referrible to a common cause, and if there be in every case an over-fulness of the abdominal epigastric vessels, it is highly probable that such a state of the vessels constitutes that cause, inasmuch as a removal of such a state of the vessels is immediately followed by a removal of all the symptoms.

4th. If epigastric pulsations be but one of many symptoms derived from a fixed cause, which may also be said to constitute, as it were, the essence* of the complaint, such complaint should be designated, not by the name of a single symptom, but rather by a name which may be used as the sign or representative of the whole class of symptoms.

5th. As the whole class of symptoms, of which epigastric pulsations is one, may be traced up to a state of epigastric chronic congestion, or subacute inflammation; and as of these two morbid states, *epigastric congestion* may be considered the primary, there can be no impropriety in designating such a class of symptoms by such a name, especially when it leads to a rational and successful method of treatment.

The reader, perhaps, will admit, that the above conclusions are drawn without any very overstrained process of induction, and I should hope that my reviewer, after the black draught, will not still insist upon my reasonings being "*hypothetical and non-conclusive*;" if so, his gastric nerves must be in a state of "*morbid irritation*," and in need, perhaps, of a remedy, to which that talented and distinguished writer, Dr. James Johnson, of London, attaches especial value, viz.: the nitrate of silver. In minute doses, it acts like a charm, and upon our reviewer may exercise a somewhat marvellous and homœopathic effect, tranquillizing the nerves, comforting the stomach, and becalming the mind. But he has promised to leave me with the reader. I shall, therefore, bid him farewell, and turn with some pleasure to the confirmation which my views of this complaint, derive from his no less distinguished contemporary, the *British and Foreign Quarterly*, *scilicet* as follows:—

"This paper refers to a well known disease, or rather symptom of disease, which is often very distressing and *refractory to treatment*. The author does not profess to add much to what is already known of the nature and causes of it; but his communication assumes a form of some importance, from containing what he states to be a method of treatment, which has been found eminently successful in the many cases in which it has been tried.

"We believe Mr. Faussett is correct in attributing many

* The word essence is here used, simply to denote the essential part.

cases of this sort to that fruitful source of chronic malady, abdominal plethora."

I have to thank the British and Foreign Quarterly for so much fair play, and am proud to add his testimony, to the extent it goes, in confirmation of the *correctness* of my opinions.

With respect to not having "professed to add much to what is already known of the nature and causes of epigastric pulsations," a friend has suggested to me, that "the views set forth" in my paper of the nature, causes, and mode of causation of this complaint, "were such as had not been advanced before." I hold demonstration, however, to be better than pretension; and if, in tracing up this affection to its proper source, viz. abdominal congestion; if in classing it merely as one of many symptoms of a determinate complaint, or in shewing the mode of causation through the agency of the ganglionic nerves and plexuses, I have given any satisfactory explanation of the phenomena in question, the object of my paper has been fully attained, *provided that it lead to the adoption of what I know to be an efficacious method of treatment.*

With respect to the part which the ganglionic nerves and plexuses may be supposed to take in the production of epigastric pulsations, it is admitted that in the present state of our knowledge, everything connected with nervous agency must be more or less problematical. The following, however, is the line of argument which I have adopted:—

We find a remarkable interlacement of nerves upon the aorta; is it to be supposed that this curious and complicated arrangement of nerves is placed *exactly there*, without design, and irrespective of the function of the great artery, *whose coats are pierced, and interwoven with its filaments?* Undoubtedly not. It is placed there in reference to the function of this vessel, and must therefore be considered as accessory, and essential to the production of its normal, or natural pulsation.

But this pulsation ceases to be natural, and becomes inordinate. We are, *therefore*, led to contemplate the change, in some corresponding change or excitement in the nervous arrangement; this, however, we have no means of detecting—but, it being proved to us that there coexists a state of epigastric congestion, we naturally infer that it is this state of congestion which gives rise to the change in the character of the pulsation; and our inference is confirmed by the fact, that as soon as the congestion is removed, the pulsation ceases. This is the "*inflammatory theory*;" and I would ask the reader, if there be anything in it necessarily chimerical, or rather that is not probable in the highest degree? especially, when he remembers that there is a contemporaneous disturbance in the functions

of other organs, which branches from those same ganglia supply, and also contemplates the many causes for congestion which are to be found in the respective processes which these organs carry on through the medium of the circulation. But, I have gone a step farther than this, though with more reserve, and stated the following opinion, viz., that any organ in this region, and in the immediate vicinity of the artery, by becoming congested, may give rise to a change in the character of the pulsation, even without implicating the nervous interlacement in immediate contact with the vessel; because if a number of nerves, as they arise from, or are connected with the ganglia, become engaged by such congestion, the disturbance produced in their functions will be propagated to the nervous centre, and thence to the parts supplied from this source, the coats of the vessel among the number.

This proposition, I purposely separate from the former, as not being substantiated by direct proof, and as better suited for a more general inquiry. The importance, however, of investigating morbid affections by the lights of physiology and anatomy, being fully admitted at the present day, we cannot, with justice, be accused of a *fondness for speculation*, if, in selecting undoubted anatomical facts, and the well established truths of physiology, and comparing these with the results of practice, we endeavour honestly to derive from thence fixed and certain principles.

There are minor degrees of pulsations at the epigastrium, not so violent as those that have been referred to, but nevertheless inordinate, which often escape notice from not being particularly complained of by the patient, and being as it were masked by the digestive ailments with which they occur, and which appear to occasion greater distress. Does this complaint, *then*, differ from what we have been describing? Certainly not. There is a difference in its manifestation by its symptoms, but the complaint itself is essentially the same, and the practitioner who upon inquiry has ascertained the coexistence of minor inordinate pulsations, with the now more prominent group of digestive ailments, and looks upon the whole merely as indications of a state of epigastric congestion, will diagnose the morbid condition which he explores with greater accuracy, and probably treat it with better success, than he who refers its symptoms to the vague and already too general term, dyspepsia. He will also, by extricating so many cases from the multitude of those that range under such a title, and reducing them to a distinct class, be contributing to the investing medicine with certainty,* in recog-

* Though medicine can never arrive at absolute certainty, surely this makes no objection to the nearest approaches to it being sought after.

nizing necessary distinctions, and attaching to names precise ideas.

It might be further shown, that what has been described as congestion, implicating the ganglionic nerves and plexuses, and giving rise to inordinate pulsations of the aorta, together with dyspepsia, may be also so modified, as to occasion the latter, without at all producing the former. For it does not appear less likely (making every allowance for difference of structure and function) that any given derangement in the centre of those nerves should produce corresponding effects, now in one organ, now in another, and again in several, than that certain lesions of the cerebro-spinal system, should occasion in one man, some loss of sensation; in another, some loss of motion; and in a third, some loss of both. It might be here interesting to examine how many of those phases of disease, or aberrations from a healthy state, which come generally under the name of dyspepsia, depend upon particular morbid impressions made upon the ganglionic nerves and plexuses; *for*, as healthy digestion may be thought more or less dependent upon the integrity of the functions of those nerves, so must unhealthy digestion be supposed to be variously influenced by their derangement. This subject, however, would necessarily embrace many considerations, far beyond the limits of the present inquiry: therefore, to return to that which is more immediately before us, I am disposed to think that inordinate pulsations at the epigastrium (at least in a minor degree) are oftener to be met with than is generally imagined. First, from having recently met with as many as eight different cases, where the patient made no intelligible allusion whatever* to a pulsatory affection at the epigastrium, until concurrent ailments led me to inquire for, and afterwards detect this symptom; and secondly, from noticing (what must be familiar to every one,) that in the lower ranks of life, patients describe their complaints in a manner so obscure, that their meaning is very apt to be mistaken. Of this, the case of Ross† is an illustration, who called the violent beating of her aorta, "*a working of her stomach.*" Another will be heard to call it "*a sort of terrible fluttering of the stomach;*" and others, "*an impression at the heart,*" or else "*a beating and soreness*" of this organ. These descriptions, however ludicrous, may yet allow us to conclude, that if inordinate pulsations cannot be sus-

* The cases above alluded to were met with in dispensary practice, and it is curious how prominent and remarkable circumstances connected with the complaints of the poor will sometimes be entirely omitted by them in recounting their ailments, and will be afterwards as if by accident revealed.

† See Dublin Journal, July, 1837.

pected from such allusions, and happen not to exist at the moment we see the patient, *they may escape notice entirely*. I shall now briefly detail some cases, illustrative of the general subject and of the foregoing remarks :

CASE II.—James Dwyer, æt. 35, of rather a plethoric habit, and by occupation a gardener, complained of a general decline of health for not his wonted ability to labour, his countenance was depressed, had six months back ; his strength and health were failing him ; he his spirits sunken, his bowels constipated, his appetite impaired, &c. He complained also of weight and uneasiness at the epigastrium, where there was considerable fulness, together with pain on pressure. These symptoms led me to ask the patient if he ever experienced a beating or palpitation in any particular place, he immediately answered in the affirmative, and pointed his finger to the epigastrium as the seat of it : he described it as occurring every day at noon, and continuing for about three hours, after which it left him, but generally returned with severity “ as he lay down at night.” Purging, counter-irritation, by means of an antimonial plaster, and mercury, henbane, and ipecacuanha, administered daily till the mouth became a little tender, brought the patient to a state of convalescence. The inordinate pulsations have entirely ceased, and he at present feels himself in every respect better than he has been for six months previously ; in his own *parlance*, he has become “ a new man.” Leeches, though directed in this case, were not applied, the consequences of which* were, as I apprehend, that the cure was slower in progress, and that the pulsations did not as usual cease about the time that the mouth became affected. The bowels, however, were freely acted on after this period for two or three alternate mornings by means of aperient medicines, and this appears to have completed the cure. In another and more severe case of epigastric pulsations where the whole routine of treatment referred to was put in force, there was a slight return of the affection, which free but gentle purging in like manner completely removed.

CASE III.—Mary Mooney, aged about 30, in the habit of working daily in the fields, presented herself, complaining of a sense of oppression and uneasiness at the epigastrium, where she experienced pain on pressure, her countenance looked downcast, and her spirits sunken, she had not enjoyed her wonted health and vigour for some time previously, she was somewhat reduced in flesh, her bowels were costive, and she had in some degree lost her appetite. This patient had been twice

* It is not to be inferred from this, that the writer holds topical blood-letting to be in every case indispensable for effecting a cure, although very generally necessary.

been and prescribed for, with little relief, but upon the third occasion her case was more carefully entered into, when the well known group of symptoms just detailed, became more particularly recognized, and led also to the detection of pulsations at the epigastrium. The course of treatment already noticed was then put in practice in a manner suited to the case, and the result was, that the patient quickly and perfectly recovered.

CASE IV.—Charlotte Clare, a small and delicate looking woman, aged 29, was visited November 4th, and found confined to her bed. About four months previous to this she got a severe beating, the several circumstances connected with which occasioned her much distress and anxiety of mind. She also received a blow at the epigastrium, whereupon she fainted, and remained, as she states, exceedingly ill for some days afterwards; occasionally spitting up blood. She was at this time pregnant, and within the last few weeks aborted with considerable hæmorrhage. From the period that she received the beating, however, up to the time of my visit, her chief complaint was a sense of weight and constriction at the epigastrium, which she described in a remarkable manner, viz.: "as if one put his knee in at the pit of the stomach and pressed her down against something hard:" this account I took to be a little exaggerated, but, nevertheless, conceived her suffering must have been considerable when it came to be thus expressed; and the uneasiness occasioned by even slight pressure of the hand was evidently very great. She did not allude to any pulsatory affection until the question was put to her, when she described herself as being constantly subject to it, and marked out the thrill of the artery downwards towards the umbilicus, and upwards to the thorax: she stated that it commenced about a month after the receipt of the injury, and had since occurred daily, though with intermissions. Being now convinced of the nature of the case, I determined upon trying a plan of treatment applicable to a state of chronic congestion, yet in a manner suited to the extreme delicacy of the patient, whose strength was necessarily impaired by a recent miscarriage. She was directed for the ensuing week to remain as quietly as possible, and to live chiefly upon milk, gruel, arrow root, and flummery. A small quantity of magnesia was given her twice a day, after which an antimonial plaster was laid over the epigastrium, and blue pill with ipecacuanha desired to be taken as in the preceding cases, until the gums became tender; this was, however, discontinued upon some slight soreness being complained of in the throat and inner part of the cheek. Nine of the pills were at this time taken, when it was also found that the sense of constriction and weight at the epigastrium had diminished, and the inordinate pulsations entirely ceased.

In this case it was evident, there was a *direct* lesion of the ganglion of the sympathetic from a blow. "I believe," says Dr. Copeland, "from an attentive examination of the phenomena consequent upon such an injury, (*scilicet*, a blow upon the epigastrium,) that the immediate effect is produced upon these ganglia, and is analogous to that occasioned by concussion of the brain." There was *then* a direct lesion of the nerves, and with this a rupture of some small vessels: what was the consequence of such lesion? reaction, and a determination of blood to the part. This by degrees subsided into a state of chronic congestion, which, at the time I first saw the patient, was manifested by a sense of weight, and constriction at the epigastrium, and by epigastric pulsations. Now, as to what the exact state of the vessels may be in relation to the nervous ganglia, when these phenomena are produced, I do not pretend to determine: all I venture to say is, that it is a state of plethora or overfulness. Again, as to wherefore they should occur in one case and not in all?—I can only record what I have met with. It would appear, however, as if a sudden and violent vascular action in which the whole vascular system might be supposed to participate, could *not* produce the pulsatory phenomena, while a state of chronic congestion, creeping on to inflammation, so often does. The *one* may be considered a salutary process, though sometimes erring in the means, and thus missing of its end, the *other* is, perhaps, more essentially a morbid process. In the case that has been detailed, the unaided efforts of nature did not accomplish their end, viz. a complete restoration to health, and a state of chronic congestion was the consequence. In the cases that are usually recorded, nature has had the assistance of medicine; cupping, leeching, &c. have probably been employed, and therefore no such unpleasant sequelæ are found to result. These remarks have occurred to me in contemplating this case, future experience may serve to shew their fallacy or truth.

As the results of observations upon several cases are comprised in all that has been already said, it would be needless as well as tiring, for the sake of noticing shades of distinction, to enter further into detail in a paper like the present. The development, however, of a certain form of dyspepsia (exhibiting itself so often in the same manner) is so constantly to be met in conjunction with epigastric pulsations, that nothing can appear more probable, than that they take their rise from a common cause, while the disappearance of both upon the removal of that cause, entirely justifies the assumption. In venturing to assign epigastric chronic congestion, as the proximate cause or essence of the complaint, I have adduced the several

proofs within my reach, it remains for the reader to test the soundness of those proofs, and to refute or confirm them by his own observation. I cannot, however, conclude those remarks without stating the satisfaction which I have often of late felt, in being able to predicate to the patient, with some degree of confidence, his chances of a speedy recovery, and in no instance as yet have I had reason to repent the exhibition of so much certainty as to the prognosis.

Surgical Observations on Tumours, with Cases and Operations. By JOHN C. WARREN, M.D., Professor of Anatomy and Surgery in Harvard University, and Surgeon of the Massachusetts General Hospital. Boston, 1837.

WE have read with great pleasure the work of Mr. Warren, and we congratulate our Trans-Atlantic brethren on the valuable addition which it makes to the medical literature of their country.

The plan of the work we particularly approve of, and the unassuming and candid manner in which it is written, does honour to the pupil of Sir Astley Cooper. The work, professing to be "*Surgical Observations on Tumours*," is what it professes, not a category of all known tumours, but a description of those tumours actually seen, or operated upon by the author, and this includes a very large class, and were it not that he has permitted several Americanisms to creep into his otherwise nervous style, we would not scruple to speak of this work in terms of the most unqualified praise.

The arbitrary division of tumours assumed by Mr. Warren is grounded upon the tissue in which they are situated. Beginning with the tissues of the skin, he then proceeds to the cellular membrane; then to those of the muscular, fibrous, osseous, glandular, vascular, and membranous; lastly, tumours composed of different textures, and not found exclusively in any: viz. encysted tumours, and those contained in the cavity of the abdomen.

Amongst the dermoid tumours we find some new names: viz. *lepoides*, from *λεπρος*, bark, intended to designate that scaly crust or bark-like roughness of the skin, most commonly observed in the face of aged persons, and which may be mistaken for a wart. The author thus describes true *lepoides*.

"A circumscribed crust, which at first appears like a discoloured

speck, or a piece of earth. This falls off, and is renovated in the same form for many successive years. When the crust separates spontaneously, it sometimes leaves a new cuticle behind, which gradually thickens. In other cases the cutis being exposed, presents a spicular surface of unfavourable aspect. This surface throws out an ill-conditioned pus, which dries in the form of irregular scales. At length, owing to accidental irritation, or a change in the constitution from age or other cause, the cutis ulcerates slowly, and forms an incurable sore or cancer of the face. The tendency of this disease is to advance very gradually, without much pain, and without contaminating any parts but those in contact with it, so that the patient may live a number of years before he is worn out by it."

The term *keloides* is also adopted from the classification of Alibert, so called from the red, vascular processes extending from the tumour, and giving it the appearance of crabs' claws. Mr. W. although adopting, still objects to this term, on account of the same idea, though not the same appearances, having caused the adoption of the term cancer, for a very different disease; and again, because in one kind of affection which must be referred to this head, there are no such processes. Three divisions of *Keloides* are enumerated.

1st. A white permanent elevation of the skin.

2nd. The spider-like pimple of the face.

3rd. The *keloides* of Alibert.

Eiloides, (from *ελω*, to coil.) This name is peculiarly well applied as indicative of the appearance of this tumour; the description given, and the delineation which accompanies it, are peculiarly worthy of attention from their correctness. The author of this critique saw two instances of this disease at Tangier on the coast of Africa, in the year 1835. Yet as they were new to him, and he had not seen any description of them amongst the writers on cutaneous diseases, he regarded them at the time as curious anomalies produced by accidental circumstances. However, the description now given by Mr. W. is confirmative of the existence of this affection as a distinct disease, and has given to it "a local habitation and a name." It is thus described:

"A true production of the cutis, and continues so throughout its growth, although it may become very large. The first appearance of it is an elevation of the skin, similar to that from a burn; without pain, redness, heat, or ulceration, it increases to a great size; and ultimately affects the patient's health. If removed it is reproduced; no cause is known, but a scrofulous habit seems to predispose to it."

The specimen of this disease removed by Mr. W. had the

appearance "of a triple coil of inflated intestine, each roll of inflated intestine, being four inches long, and growing by a narrow base from the side of the neck. It occurred in a negress." This disease has as yet been of too rare an occurrence, or too inaccurately observed, to enable us to give any very decided opinion on the subject; but yet it is easy to distinguish it from the molluscum of Bateman, which is an atheromatous disease, proceeding to ulceration before it has attained any very great size, and then exuding a white milky fluid capable of producing the same disease in others by contact, and occurring in many places on the same person. Eiloides, on the contrary, as far as has been observed, is a disease of a solitary nature, peculiar in its form, its main characteristic being the curious snake-like coils which it assumes, appearing to consist of a simple thickening of the skin, capable of arriving at any size, and scarcely affecting the health of the patient except by its weight; occurring in all the cases as yet remarked, on the side of the neck; and from the evidence of Mr. W. it appears that the cicatrix remaining after operation, is capable of assuming a form and appearance similar to the original tumour. Perhaps it might be placed with some degree of propriety under the head of the elephantiasis tuberculata of Alibert. In any case the profession is indebted to Mr. Warren for his notice of this disease, as it may draw the attention of some of our intelligent army or navy surgeons, on foreign service, to its more accurate investigation.

It would be impossible, in a review of this kind, to notice each article separately, we will therefore pass on, touching merely on those points which we conceive to possess originality in some of the articles.

Our author speaking of the division of the sphincter ani for fissure of the rectum, says:—

"In cases where the fissure has been in the side of the rectum, I have not divided the fissure, but made the incision towards the os coccygis, from its greater facility. The object being to take off the tension of the sphincter, and thus give the fissure an opportunity of healing, it is not important where the incision is made, provided there be a sufficient division of the sphincter."

We would suggest at the same time potential cautery to the fissure to insure its union. Although we cannot at once coincide with the practice recommended, as the dressing of two fissures must be more difficult than one, and the facility which accrues to the surgeon of cutting towards the os coccygis should not for a moment be entertained, except in circumstances where he could not procure a proper assistant.

In treating on tumours of the fibrous structure, an interesting case is detailed :—

“ The patient was a clerk or writer, 18 years old, possessed of great muscular power. In the summer of 1834, he undertook to raise himself from the ground, while sitting with his lower extremities extended; and this was to be done without using his hands. This feat he succeeded in accomplishing. No immediate inconvenience was recollected. Two or three weeks after this act, he perceived a weakness about the knees, and examining found a slight hardness along the tendons of the flexor muscles. He continued to move about, but found the tumours and lameness regularly increasing. It was three months after the effort spoken of before he applied to me. On examining him, there was felt along the tendons of the biceps muscles in both limbs, a hardness of about four inches in length, and near an inch in thickness, principally seated on the inner edge of the tendon, and partly on its posterior face. When the muscles were contracted by an effort to raise the leg, the patient lying on his face, the tumour became of a stony hardness; when the muscles were relaxed, it was less obvious. No distinct uneasiness was caused by compressing the part. At this period he was unable to walk more than a few steps. After considering his case, I told him, it was of a difficult and protracted nature, and that most patients became tired of the treatment necessary in such a case, and applied to bone and sinew doctors, which course, if he had any disposition to adopt, he had better do it at once, without giving himself or me the trouble of beginning a treatment, he would probably not carry through. He then informed me, he had already made a trial of the practice mentioned, and found himself growing worse, and being now seriously apprehensive of being crippled for life, he was ready to submit to the requisite treatment. He was then informed, that he must prepare himself for a confinement of at least a year, possibly of two years or more. He was directed to keep in a horizontal posture on a bed; the knees to be slightly flexed, and supported in this posture, and never to be moved excepting passively. Twelve to eighteen leeches every fifth day for the first six weeks. Occasionally a purgative. No animal food, and the quantity of food limited. I heard no more of him for five months. He then sent for me, I found him pale and emaciated. He had severe pains in the bowels, and frequent bleedings at the nose. He had followed the course directed in every particular. I expressed my surprise, that he should have undertaken to proceed so long a time, without giving me an opportunity of accommodating the treatment to circumstances. He apologized by intimating, that being compelled to relinquish his occupation, he was without means of defraying more than the expenses of subsistence. From this time I attended him regularly. I should have mentioned that the tumours were sensibly diminished. He was now directed to take a little meat twice a day, to drink from four to six ounces of wine, and forty drops

of tincture of sulphate of quinine every two hours. Whenever the abdominal pains were urgent, to take thirty drops of tincture of opium. Under this management he recruited slowly, and at the end of four weeks, the wine gave him headach, and was discontinued. Animal food was continued a month longer, and then seeming to disagree with him, was omitted. He now resumed the original course, with the exception of substituting injections for purgatives, and sometimes taking a little meat. After eight months' confinement he was directed to use frictions of a stimulating liniment, and to have the limbs bathed twice a day, in hot water and soap. Finding the tumour lessening regularly when he had been confined a year, I directed him to bandage the whole limb; and to get out of bed once a day. This was followed, after two or three week's trial, with a thickening of the cellular substance between the hamstring muscles; in consequence of which he was put on the bed for a month longer, and then advised to get up and walk the room once a day. At the end of fifteen months, he was able to walk a quarter of an hour at a time. I now urged him to walk out, but his apprehension of reproducing the disease was so strong, that it was some time before this could be affected. At last he got out, and increasing his walks very gradually, in three months he was able to walk to my house; and I had the satisfaction to find, that the swellings were wholly dispersed. Since then he has recovered his strength, and is now perfectly well."

Among the cases of periosteal tumours, one is remarkable from the practice pursued in the operation.

"The tumour was on the right side of the face, extending from the edge of the lower jaw to the zygomatic arch, having the size and nearly the form of a lemon. It had existed a year; came on without any known cause; was colourless, quite hard; not attended with much pain, and made a formidable appearance. The patient's health was perfectly good."

Having determined that it was neither exostosis, osteosarcoma, nor fungus of the antrum, Mr. W. proceeds: that perceiving the breath to be offensive, he examined the mouth, and at length discovered a small aperture leading to the lower jaw.

"Introducing a probe, I struck a rough bone, and was at once satisfied of the nature of the case. I made a perpendicular incision about three inches long, and laid bare the masseter; after dissecting through this muscle, a semicartilaginous substance appeared; the knife with difficulty penetrated this, and being forced to some depth, did not reach anything of a different character. I then proceeded to excise this hard substance; and neglecting the circumference, cut out a mass an inch and a half square, and an inch thick, from its centre. This being removed, a probe was passed, and struck a diseased bone. The aperture was then enlarged, a strong forceps introduced, and with some difficulty a dead piece of the lower jaw was

extracted, about two inches long, and comprising the whole mass of the lower jaw bone for that extent. The wound healed favourably though slowly; the relics of the hard substance gradually disappeared, and the patient was discharged in three weeks after the operation. The extraordinary thickening in this instance, was caused by the effort of nature to supply the deficiency of the dead bone, connect together the separated parts of the jaw, and keep up its motions, which were never suspended, though much limited. In this case, as well as others of periosteal tumour, an extraordinary sensibility exists in the thickened periosteum. This symptom was remarked in most of the cases mentioned; and in necrosis, where it has been necessary to remove portions of thickened periosteum, to expose a dead bone, the patient has always seemed to suffer excessive pain from cutting this part."

Cases are given in which portions of ribs were removed, both with the chain saw and cutting forceps, without injury to the pleura, well worthy the attention of the practical surgeon.

Mr. W. mentions one curious preparation, as being in his possession. An exostosis of the periosteum of the femur, which encircled the bone like a ring, and which when the periosteum became dry, revolved easily on its shaft. At the same time, it appears to have caused absorption, as the cylinder of the bone was furrowed beneath it.

Several operations are described for the removal of the upper jaw, for maxillary exostosis, in which cutting forceps were employed (in the manner described by Mr. Liston) so far back as May, 1831. Then Mr. W. made the first use of this instrument; since that period he states that great improvements have been made in modifying the instrument by Dr. Flagg, who gave some of them the form of a hawk's bill; one of them is made to cut horizontally, another vertically: thus, the two incisions necessary for the removal of a portion of the jaw can be made rapidly, and with ease. The French instrument, which is made to cut at right angles with its shaft, is also mentioned in terms of commendation; and a proposal is made to bring in the aid of an additional lever in the construction of a compound forceps, which would give a power of surmounting any resistance which an osseous tumour could give, and thus supersede the use of the saw. Of this idea we most highly approve, and we feel certain that every candid person who has seen the difference between the operation as performed formerly with chain, bow, spring, or Hey's saws, and that of the present day with the cutting forceps, would most cordially acquiesce in the opinion of Mr. Warren.

In osteo-sarcoma of the ribs our author speaks fearlessly of

their excision along with the tumour, and cites cases in which he has performed this operation; here as well as in many other parts of the work, we were pleased at the acknowledgment which he makes, that the surgeon must be ruled by circumstances at the time of operation, without regard to rules previously laid down by himself or others, if he wishes to adopt the course which will be most beneficial to his patient; very many of the cases of operation give evidence of this, although in all the plan of operation, with the presumed anatomical difficulties, were fully considered before hand; still the Doctor seems seldom to have deliberated long, when his first incisions shewed that a change in his original plan would be more beneficial. One example from many will suffice. The author having determined on the removal of an osteosarcomatous tumour of the inferior ribs, and the first parts of the operation having been performed to discover the connexion of the tumour with the ribs, he thus proceeds:—

“ I uncovered a small portion of the ninth and tenth ribs, and soon ascertained that the tumour originated in the ninth, and was adherent to the seventh, eighth and tenth. The knife was insinuated between the tumour and these three ribs, with some caution on account of the danger of penetrating the intercostals; and now the tumour being separated from these, was perceived connected with the ninth only. A question arose whether it was best to divide the rib first or to take off the tumour. Ascertaining that by removing the tumour, I could save an inch of the rib by dividing it under the tumour, I cut off the tumour with an inch of the cartilage of the rib, and then having the morbid origin of the tumour in full view, proceeded to remove the rib. The intercostals above and below the rib were carefully divided with the knife. The intercostal artery, vein, and nerve were separated with the pleura. A small director was then insinuated between the pleura and bone; and on the director the rib was cut with the cutting forceps applied to the edge of the rib. The diseased portion was then dissected from the pleura and diaphragm, with a portion of the cartilage of the rib. The exposed diaphragm rose into the aperture of the wound, forming a sort of hernia. The hemorrhage was not great.”

Contrary to the expectations of Mr. W. there was no thickening of the pleura in this case, which had it existed would have been a kind of security in separating the rib. This case, which was successful, concludes with the following remark:—

“ I would not, however, be willing to have it understood that the raising of a rib from the pleura or diaphragm is a light and easy operation. It is one of great danger both in the execution and the con-

sequence, if the pleura retain its healthy state; but if it be thickened, the danger and difficulty is much diminished."

In the article on glandular tumours, a description is given of an immense tumour extending from the ear to the clavicle, and including in its substance the carotid artery, internal jugular vein and par vagum nerve, and extending deeply into the parotid space. This tumour was removed by operation, the nerves and blood-vessels having been dissected out of it, with the exception of the sublingual nerve, which was cut across. As far as the operation and its consequences were concerned the case terminated successfully, the patient dying some months later of ulceration of his throat. After perusing this case, a question naturally arises in the mind of the reader, was the glandular scirrhus removed, a scirrhus enlargement of the parotid gland, or only of that small gland usually imbedded in its substance? The term parotid never occurs in the whole description. Speaking of the hereditary nature of cancer, a point on which Mr. W. lays great stress, he says:—

"The following instances have occurred within my knowledge, to a family in this vicinity. The grandfather died of cancer of the lip; the son had a cancer of the breast, and at the age of sixty was operated on by my late father, but died of cancer a few years after. Two of his sisters had a cancer of the breast, were operated on, and ultimately died of the disease. A daughter of one of the ladies had a cancer of the breast, which I removed at an early period; she recovered, but died some years after of disease of the uterus. A daughter of the gentleman has a cancer of the breast. I have reason to believe that other members of the family are affected, and conceal the existence of the disease."

A very strange instance is related of the effect of imagination on the physical frame:—

"Some time since a female presented herself with a serious enlargement of the submaxillary gland, which had become troublesome and alarming to her. I considered any attempt to dissipate it by medicine to be vain, and advised its removal by an operation. She would not consent, and to gratify her, I directed some applications of considerable activity to be made to the part, and these she pursued a number of weeks without any change. After this she called upon me, and with some hesitation begged to know if an application recommended to her would in my opinion be safe. This consisted in applying the hand of a dead man three times to the part. One of her neighbours now lay dead, and she had an opportunity of trying the experiment, if not thought dangerous. At first I was disposed to divert her from it; but recollecting the power of the imagination, I gravely assured her she might make the trial, without apprehension of serious consequences.

A while after she presented herself once more, and with a smiling countenance informed me she had used this remedy since I saw her, and no other, and on examining for the tumour I found it had disappeared."

In cancer of the uterus, the author gives it as his opinion, that

"Excision of the os uteri, may be safely and properly done in some cases. The extirpation of the body of the organ, I cannot recommend. I have indeed performed it once, but should not do it again. The size of the uterine vessels, their inaccessible situation, the existence of circumstances favourable for maintaining hæmorrhage, and the injury to the peritoneum, are facts which seem to me to place this disease beyond the hope of successful operation."

Passing from thence to the diseases of the male organs of generation, the author in more than one passage alludes to the constant use of tobacco, as one of the causes of various diseases; amongst others, it is mentioned as having an injurious influence on the spermatic veins, and if not alone causing varicocele, still cooperating with other causes in producing it.

In the early stages of varicocele, "immersion of the scrotum in iced water" is recommended,

"Until a sense of pain is brought on; and this to be repeated three times a day. The scrotum to be kept as cool as possible, and always supported by a suspensory bandage."

Amongst the various modes of treating varicocele, Mr. W. states, that he never had been able to prevail on patients to submit long enough to the system of compression employed by M. Breschet, of Paris, to enable him to judge of its utility. This method is exceedingly painful, consisting in compression of the veins in a small iron vice, till inflammation and consequent obliteration of the veins is caused. He states, that ligature of the artery is often had recourse to in America, and he seems to think that such practice is not eminently successful. Excision of the spermatic veins, he states to have been his ordinary practice, and that its results had been favourable.

A case of hydrocele, recorded at page 499, proves that the author was not fond of mere palliative measures:—

"A gentleman affected with hydrocele applied to me under the following circumstances. He had been affected for ten years, and the fluid had been evacuated only two or three times. He determined to make a voyage to England for the purpose of undergoing a radical operation. While in England he was so situated as to be unable to go through the operation, and left England for Boston to have it effected here. When he arrived, his hydrocele was large enough

to hold a quart of water; too hard to distinguish the testicle, and perfectly opaque. The undulation of water could be perceived, he had been told that injection was the proper mode of operating, and he wished me to practice it, which I therefore did. The fluid of the hydrocele being discharged by trochar and canula, near a quart of port wine and water was injected without exciting pain. This being evacuated, as much port wine, with a quarter part brandy, was used. No pain being brought on, a pint and a half of brandy, with two drachms of sulphate of zinc, was injected. This caused a little pain, and the injection having been thrice practised, I would not proceed further with it, though satisfied that it would not be followed with sufficient inflammation. The next day, however, the scrotum swelled, and he had a little fever. On the third day the swelling was rapidly abating, and so continued the next two days without pain. Convinced that adhesion would not be effected, and perceiving the great inconvenience he would suffer from waiting the result two or three weeks longer, I prevailed on him to submit to incision. The operation was directly performed by laying open the whole length of the vaginal sac. As soon as this was done, a little water issued. The tunica vaginalis was in hardness almost cartilaginous. On looking into the sac, I saw a most delicate network of lymph filling the space between the testicle and tunic. This lymphatic exudation formed large cells, containing abundance of water; with the finger and a probe I broke away all this lymph, cleared out the water, and placed a piece of lint in contact with the testicle, the full length of the wound. In this way the requisite irritation was produced. In seven days the lint was drawn away; and in three weeks from the period of the incision, he was perfectly well."

Three instances are given of air getting into a vein during operation, one of which, attended with the most frightful symptoms, (see page 260,) terminated almost immediately fatally, notwithstanding the most powerful restoratives and stimulants being applied, and even tracheotomy had recourse to as a *dernier ressort*.

There are many parts of this really valuable work, which want of space, rather than want of inclination, prevents us laying before our readers, or even touching on in a cursory manner. We could have wished that the author had been a little more particular in the phraseology which he employed in many parts of his book; we give a few examples that his attention being directed to the subject, he may be enabled to correct them in the next edition; page 36, "The next time I saw Miss B. was last August, when you *was* consulted;" p. 108, "A few days ago I met him in the street, and had the pleasure of hearing that he was perfectly well, and quite *rugged* and strong;" p. 186, "Had agitated him some;" p. 207, "Was found to have between twelve and thirteen pounds, &c.;" p. 227,

“ With quite a severe constitutional disturbance ;” p. 306, “ She was seized with a bad chill ;” p. 474, “ I lately operated on a young woman woman who had quite a large polypus,” &c. We would not remark those trivial matters, which in a really valuable work are but as “ specks on the snow wreath,” were it not that we wish to preserve the English language in its utmost purity. Considering the immense extent over which it is spoken at present, it is not irrational to imagine that at some future period, it may form *par excellence* the medium of communication for the whole globe. It is, therefore, if for no other reason, important to prevent any incorrectness or misapplication of terms, or even that useless redundancy of phraseology, (a fault to which many of our American brethren are prone), from creeping silently and unremarked into its scientific works.

S. L. L. B.

SCIENTIFIC INTELLIGENCE.

Second Report of the London Committee of the British Association for the Advancement of Science, on the Motions and Sounds of the Heart; communicated at the meeting at Liverpool, September, 1837.—The Committee appointed in London to investigate the motions and sounds of the heart, have to present to the Association a short account of some investigations of the abnormal sounds of the heart and arteries, in which they have been recently occupied, and which were not comprehended in their former report.

Before describing these, the committee would remark, that although their inquiries have not been specially directed to that subject, yet they have had many opportunities of verifying the conclusions on the *natural* sounds, as presented in their report of last year; and these conclusions not having been since shaken by any experiment or rational objection, it may be considered as fairly established, that the first, or systolic sound of the heart, is *essentially* caused by the sudden and forcible tightening of the muscular fibres of the ventricles when they contract; and that the second sound, which accompanies the diastole of the ventricles, depends solely on the reaction of the arterial columns of blood on the semilunar valves at the arterial orifices. It further appears, that the first sound may be increased by an additional sound of impulsion against the walls of the chest under certain circumstances of posture, of increased action of the heart, and of particular stages of the respiratory act. It is also obvious, that the character of the first sound may in some measure depend on the closure of the auriculo-ventricular valves, and on the quantity of blood, inasmuch as these determine the nature and time of the resistance, against which the muscular fibres of the ventricles tighten. So likewise the vigour of the ventricular systole, the quantity of blood propelled by it, the sudden and complete character of the diastole, the fulness of the arterial trunks, as well as the perfect, mobile, and membranous condition of the semilunar valves, will determine the character and loudness of the second sound. An experimental illustration of one of those conditions was observed by the committee during one of their experiments on the ass—a great diminution of the second sound on the carotid artery being freely divided.

As additional illustrations of the production of a sound like that of the heart, by muscular contraction, the committee have noticed that accompanying the action of the paniculous carnosus of the ass during life, and the quivering contraction of muscles immediately after death. The sound produced in the latter case, in nature and frequency, closely resembled the first sound of the heart of the foetus or of small animals.

In investigating the morbid sounds of the heart, the attention of the committee has been chiefly directed to the causes of those remarkable and various accompaniments of the heart's action, called murmurs, which were happily compared by Laennec with the noises of blowing, filing, rasping, sawing, parring, cooing, &c. This inquiry consists of two parts: 1. *What is the essential physical cause of the phenomena in question?* and 2. *How does the apparatus of the circulation developethis cause in the various instances in which these phenomena are known to occur?* To the first of these inquiries, the experiments of the committee supply what they trust will be deemed a satisfactory answer: the second is to be answered by extensive clinical and pathological observation, rather than by direct experiment; and although a few physiological experiments will be quoted to this point, yet the committee do not profess at present to do more than open this inquiry to all those who have the means of pursuing it.

Experiments on the Production of Sound by the Motion of Water through a Tube.—1. A caoutchouc tube, eighteen inches long, and three-eighths of an inch in diameter, was attached to the stop-cock of a reservoir, in which there was water to the depth of from eight to ten inches.

When the water flowed perpendicularly through this tube (the air being first expelled,* and the lower end of the tube kept under water in the recipient below,) no sound was heard; but on pressing any part of the tube, so as to diminish its calibre, a blowing sound was heard at and below the point of pressure, and this sound became louder and more whizzing as the pressure was increased. The loudest sounds were obtained at the lowest end of the tube, where they were sometimes quite musical; and by increasing the pressure at regular intervals, a periodic increase and raising of the sound was produced, which closely resembled the sound heard in the neck, to which the French have given the name of "bruit-de-diable."

2. A pin being stuck transversely through the tube, a slight blowing was heard. A similar phenomenon more distinctly resulted from the use of a split goose quill placed in the same way. A stronger blowing was produced by two threads across the diameter of the tube, especially when they were rather loose; and a still louder and

* As long as any air remains in the tube, a loud crepitation accompanies the current. In most of these experiments a flexible ear-tube, without its cone, was used, as being more easily applied than the common stethoscope.

shriller sound ensued when a knot of string was fastened to the threads.

3. The same tube being adapted to the stop-cock of a water supply-pipe, through which the current could be let to pass with great force, it was found possible to imitate every variety of blowing, whizzing, and musical murmurs, by varying the pressure on, or obstruction in, the tube, and by altering the force of the current. When the current was strong, the least obstruction caused a murmur, but with weaker currents, greater obstructions became necessary for the same effect. An obstruction, which, with a weak current gave a blowing sound, produced with a stronger current a sound of a more whizzing character. Grating or rasping sounds were best obtained by the effect of a strong current on a knotted thread across the diameter of the tube. The musical or uniform sounds resulted from a moderately strong current through a considerable impediment; increasing the force of the current or the degree of obstruction, rendered them whizzing and imperfect; diminishing the current or the obstruction, converted them into a simple blowing. When a sound was of an appreciable pitch, its note was high in proportion to the force of the current and the amount of the obstruction,—a fine forcible stream producing the highest note. Sometimes, however, with a strong current, a loud trumpet note would be set up, which was not altered in pitch, but only in force, by changing the strength of the current. This kind of note produced visible vibrations of the tube below the obstruction, and seemed to have relation to the length of the tube. In many instances these vibrations resembled closely the purring tremor and thrilling vibration sometimes felt in the region of the heart and large arteries. Musical sounds of a more variable character, like the cooing of a dove, the humming of an insect, or the whistling of wind, were produced with weak currents passing through a tube much obstructed. The pressure of a column of water only two or three inches high, was sufficient to give acute whistling notes, which were sustained, although varying, even when the water that passed, only fell in drops from the end of the tube.

4. Bending the tube to an angle produced a murmur; but no sound resulted from any curve that did not infringe on the calibre of the tube. A circular constriction, by a thread drawn round the tube, caused a murmur, which was blowing or whizzing according to the strength of the current.

5. The current issuing from the end of a tube, or from the mouth of an India-rubber bottle, produced a blowing sound when it impinged directly on an opposite surface, such as the side of the recipient or the end of the stethoscope; but unless the current were very strong, this sound was not produced when the current played on the surface very obliquely.

6. When the tube, with a weak current, was pressed on at two points, the murmur was heard at the point where the pressure was greatest; and by increasing the pressure at one point, the sound was stopped at the other. When the current was strong, it was easy, by a pretty equal pressure, to cause a murmur at both points.

7. With a strong caoutchouc tube, two feet long, and one inch in internal diameter, the same results were observed, but in a more remarkable degree, in consequence of the increased size of the tube. When the current was strong and the pressure on the tube considerable, sounds were produced loud enough to be heard without applying the stethoscope to the ear, and the vibrations of the tube below the obstructions were so strong, that they threw the water in little jets from the outside of the tube.

8. In making the last experiment, the pressure of the water suddenly distended a portion of the tube into a globe about three inches in diameter, constituting a good representation of a circumscribed true aneurism. As long as the force of the current was sufficient to keep the walls of the dilated portion distended, no murmur was produced; but when these walls became flaccid, the passing current caused a kind of dull fremitus in them. Slight pressure on the dilatation or bending the tube to form an angle at the point, also sometimes occasioned a murmur.

9. A globular India-rubber bottle, three inches in diameter, being adapted to an aperture in the side of a tube, so as to form an elastic sac communicating with it, and the air being expelled, the current of water was directed through the tube. The same was done with a smaller tube, and a bottle one inch and a half in diameter. In some positions the current, in passing the lateral sac, occasioned a slight whizzing; but in others, as when the tube was straight, there was no sound. A sudden increase of current, or the removal of pressure from the sac, caused a whizzing, by the entry of water into the sac. Independently of the current, sudden forcible pressure on the sac occasioned a whizzing with the expulsion of the fluid; and a similar whizzing attended its rapid reflux into the sac on the removal of the pressure.

10. Some of the preceding experiments being repeated with water made slightly glutinous with size, it was found that the various sounds were not quite so readily produced as with plain water, and required a greater force of current.

From all these results (1, 2, 3, 4, 5, 6, &c.) it is sufficiently plain that a certain resistance or impediment to a liquid current is the essential physical cause of all murmurs produced by the motion of fluids in elastic tubes. That any condition of the walls of the tube beyond the obstructing point is not, as it has been supposed, essential to the production of these sounds, is proved by the fact, that they may be produced by an obstruction at the *terminal orifice* of the tube, or at the *mouth* of a gum-elastic bottle, where there is *no tube or wall beyond* to cause them (1, 5); and usually this is the situation where they can be produced best, because here the current has acquired its greatest momentum, and finds a freer passage beyond the obstructed point. The more flaccid state of the portion of a tube beyond an obstruction, is a necessary effect of the impediment caused by that obstruction to the passage of water. It is therefore a necessary *concomitant* of the obstruction, but it is not the *cause of the sound*. When, however, the

sound occasioned by the obstruction is strong, its vibrations may be communicated to the whole contents and walls of the tube beyond (3, 7,) which will then vibrate in system with it, and be capable of modifying its note; just as the tube of a reed instrument *affects* the note, which is *generated exclusively in the reed*. On the other hand, when the sound is weak and varying, the condition of the tube or walls beyond it will not affect it. In short, the laws of the production of sound by liquids, so closely resemble those which regulate the same phenomena in air, that it is unnecessary to enter into further detail respecting them. It may be necessary to advert to an objection to this view, that a murmur is sometimes caused where there is no impediment to the course of a liquid, as when it passes suddenly from a small into a large tube or into a sac. In the first place, it is not quite correct to assume that in this case there is no impediment; for the liquid in the large tube or sac having less velocity than that in the small one, must itself be an impediment. But besides this, the course of the small swift current becomes changed by spreading into the larger channel, and instead of running smoothly parallel to the tube, now strikes its walls at an angle, causing a series of impulses and resistances which, if forcible and rapid enough, constitute the vibrations of sound. It is in the same way that a current produces a sound by impinging against an opposed surface (5.) It may be observed, however, that these indirect kinds of impediment to a moving current, are not so constantly attended with the production of sound, as the direct obstacle presented by a narrowing of, or projection into, the calibre of the tube.

Experiments on the Production of Murmurs in the living Body.—11. About two inches length of the common carotid artery of a young donkey was laid bare. Different degrees of pressure, either by the stethoscope, or by a probe pushed under the artery, occasioned a variety of murmurs, blowing, sawing, filing, and musical or cooing at each pulse. When the stethoscope was merely placed in contact without pressure, no murmur was heard, but only a simple impulse and sound, which was distinct only when the heart acted strongly.

12. The artery was scratched for a few seconds with the point of a scalpel; it gradually became sensibly smaller for the length of half an inch about that point. A strong solution of salt being applied, the contraction increased; but it was still of a gradual and tapering kind, and the stethoscope could not detect any murmur in it; very slight pressure was enough to cause a whizzing. The pulse at this contracted portion was felt to be much harder and sharper than above or below it.

13. A small incision being made into the artery, a jet of blood issued, and a whizzing, sometimes continuous like the bruit-de-diable, sometimes only in pulses, was heard beyond the orifice, but no sound on that side of the orifice nearest the heart; the sound being, as usual, carried in the direction of the current. The incision being made larger, the blood spouted out with whizzing to the distance of more than six feet, and the animal died in the course of ten

minutes. After this last incision the beats of the heart were frequent, short, and pretty loud, but without a second sound, and without any murmur to the last. They continued for nearly two minutes after the respiration and consciousness had ceased, becoming gradually slower.

14. The Committee repeated the observation that has often been made before, that a murmur can easily be produced by pressure on the subclavian, carotid, or femoral artery of the human subject. This murmur is generally of a grating or filing character, and is prolonged in proportion to the degree of pressure.

15. Whilst making the observations on the carotid, they found that a continuous murmur of very remarkable and variable characters could be produced by pressure on the jugular veins. The most common sound thus produced was like the humming of a gnat or fly, but occasionally it resembled the whistling of wind, the singing of a kettle, the cooing of a dove, and sometimes it was perfectly the remittent whirring noise, which the French have called the *bruit-de-diable*. Dr. Ogier Ward, of Bermingham, had previously come to the conclusion that this sound is produced in the jugular veins; and the observations of the Committee confirm this inference; but they do not agree with this physician in the opinion which he adopts from MM. Andral and Bouillaud, that the presence of these sounds always denotes a chlorotic state of the system, in which steel is indicated, or that they are essentially morbid symptoms at all. They may be produced in the healthiest subjects by moderate pressure applied to the lower part of the jugular veins, and are then found to be modified by various circumstances, which can only affect the venous current. Thus, they may be arrested or diminished by pressure on the vein above, by the horizontal posture, or hanging down the head, and by forced efforts to expire with the glottis closed. They may be restored in increased degree by suddenly desisting from any of these acts or circumstances. The occasional pulsatory or remittent character of these sounds seems to depend on the momentary increase of pressure caused by each pulse of the neighbouring artery; and when, as it sometimes happens, these pulses are attended with a whizzing, this is in a measure incorporated with the venous sound, and increases its periodic swell. The size and downward current of the jugular vein peculiarly adapt it for the production of sound, but it is probable that sounds may be produced by pressure on other veins, when circumstances accelerate the current within them. The Committee have succeeded in detecting an obscure murmur in some of the large superficial veins in the arm and thigh. It is to be distinguished from muscular sound, which it resembles, by its being heard only when the small end of the stethoscope is applied on a vein, and by its being stopped by pressure on that vein. A louder continuous sound is sometimes heard at each side of the upper part of the sternum, which, from its resemblance to these venous sounds, and from its being stopped by forcible expiration, may be supposed to have its seat in the large venous trunks underneath.

Although it appears from these facts that the venous sounds are

not necessarily signs of disease, yet the circumstance proved by the Committee (10), that water is thrown into sonorous vibrations more readily than a liquid of a more glutinous character, renders it probable that these, and other sounds depending on the motion of liquids in the apparatus of the circulation, may be more easily produced where the blood is thin and deficient in quantity; and under these circumstances they may occur in the neck from the mere pressure of the muscles on the jugular veins.

The Committee had planned several experiments for the further elucidation of the second part of the inquiry, namely, *by what changes functional and structural, does the apparatus of the circulation develop the physical causes of abnormal murmurs and sounds, in various instances in which they are known to occur?* Having failed in obtaining animals in time for this Report, the Committee propose to resume at a future time this part of the inquiry, so important for the elucidation of several obscure points in pathology, diagnosis, and practice; and to report the result of their labours at the next meeting, if the Association should think fit to reappoint them for that purpose.*

(Signed)

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Lond. Med. Gaz., Dec. 21, 1837.

Royal Medical and Chirurgical Society, Nov. 28th, 1837. The President in the Chair. *Removal of the Clavicle with a Tumour situated in that Bone*, by Benjamin Travers, F.R.S., Senior Surgeon to St. Thomas's Hospital.—The patient in this case was a young gentleman, æt. 10, a native of the East Indies, but resident for the last five years in England. In the summer of 1836, a swelling, of the size of a hedge-nut, was observed in the centre of the left clavicle, which was attributed to a fall he had had ten days previously, and treated accordingly. At about two months after this discovery the author was consulted, when the swelling had attained the size of a pigeon's egg, was elastic, and painful when compressed. Various means were had recourse to with the view of dispersing the tumour, but without avail, as by May, in the present year, its base, from the scapular extremity, occupied 3-4ths of the bone. About 2-3rds of its bulk were supraclavicular, so that in the erect posture it was seen far behind. It being determined to remove the bone with the tumour, the operation was performed on the 6th of June, twelve months after the commencement of the disease. The operation was effected without difficulty, and no considerable blood vessels were injured. The entire clavicle was removed, with the exception of a small portion of its sternal extremity. At the date of the present narrative (Oct. 1837) the wound had been very completely

* The Committee was reappointed, and £25 placed at their disposal for the further prosecution of these researches.

cicatrizized; there was no falling forward of the shoulder, nor any restriction of the motions of the arm: indeed, such facility does the patient enjoy in this respect, that one of his amusements is rowing a boat upon the Thames. There is a production of bone from the truncated extremity of the clavicle, extending at least two inches, and terminating beneath the centre of the cicatrix. A section of the tumour, in its longest diameter, presented an arrangement of cells filled with a dark, solid coagula of blood.

The author conceives that there can be no reasonable doubt of two points, viz. that the tumour began in the cellular structure, and that it was the result, direct, or indirect, of the fall. The explanation offered by the author is, that a medullary extravasation had taken place from the concussion attending the fall, or from the fracture within the periosteum, in either of which cases the effused blood which, in a perfect solution of continuity, would have been absorbed, acted as a foreign body; and by effectually stopping the osseous secretion and starving the bone, became the instrument of the absorbing process. He is induced, by the phenomena of such cases as the present, to entertain an opinion, that many anomalous fixed tumours on record arise from changes within the medullary canal of the bones, and are exasperated into intractableness by their confined situation. The paper concludes with a categorical summary of the diseases of bone, which the author's observation and the records of surgery enable him to verify.—*Ibid.*

St. Bartholomew's Hospital, Tuliacotian Operation.—Sarah Jackson, æt. 20, a native of Hertford, came up to St. Bartholomew's Hospital, in the beginning of last August, to undergo an operation for the reparation of her nose, which had been destroyed three years previously, by lupus. The disease commenced when she was a child, eight years old, on the right cheek, and gradually extending its ravages to the integuments of the alæ and septum of the nose, destroyed the cartilages of the apertures, and removed the whole of the integument as high as the triangular portion, which it left subcutaneous. A small part at the anterior and posterior part of the fleshy columna still remained. After a time, the left side of the face became similarly involved, and medical aid was unable to arrest the progress of the disease for a period of eight years. The face at this time presented an irregular rugous appearance, much thickened from a deposit of lymph, with patches of bright-red vessels ramifying in the skin, and had an unhealthy aspect, although quite free from any appearance of the disease itself. She was admitted into the hospital, under the care of Mr. Stanley, who, after a careful examination, fearful of the operation again lighting up the original disease, refused to subject the patient to the liability of its return.

In this conclusion he was somewhat strengthened by Professor Dieffenbach, of Berlin, who visited the institution about this time. He was of opinion, that although it was an unfavourable case, there were still sufficient grounds to warrant the performance of an opera-

tion. Personal appearance being an important consideration with the young woman, she was importunate in her solicitations to take advantage of the opportunity surgical means offered; and after being duly informed by Mr. Skey of the liability of failure (to whose care she had been previously assigned), the operation was determined on, and performed on Saturday, the 21st of October.

The first step consisted in paring off the skin on both sides the alæ, over a surface of about one-third of an inch wide; to which treatment also the anterior and posterior part of the columna nasi were subjected. A perpendicular incision was then made down the bridge of the nose, commencing a little above the junction of the frontal with the nasal bones, and the sides were reflected sufficiently to admit of the insertion of the stalk of the flap brought down from the forehead. A low state of the forehead rendered it necessary to divide the flap of integument obliquely; the direction of it, therefore, was upwards, outwards, and to the right side. It was of a crucial shape, consisting of three portions situated at right angles to each other: a perpendicular portion, or tongue, from half an inch to three quarters of an inch in diameter, to be applied to the septum, and two lateral ones, of rather larger dimensions, to be adapted to either ala. The meeting of these formed the prominence of the nose. The tongue, or perpendicular portion, advanced close upon the hair; and the traverse ones extended from the upper part of the eyebrow below, to the hair also above. A thin stalk, of an inch and a quarter long, and a quarter of an inch broad, was then carried down to meet the incision made along the dorsum of the nose. A little smart bleeding followed, and occasioned a delay of about a quarter of an hour. As soon as this was repressed, the stalk was twisted upon itself towards the right side, introduced between the two reflected portions on the dorsum of the nose, and kept in its situation by ligatures applied at proper intervals. The same method was also employed to confine in their proper situation the alar and columnar portions.

At eight o'clock the same evening the transplanted piece of integument was quite warm, but rather pale. She was conscious of its being touched, and strange to say, referred it, not to that part of the forehead from whence it was reflected, but to the very identical spot on the nose on which it was fixed. The ligatures were removed on the third day. The whole of it had united, except a small piece just at the meeting of the left alar portion with the cheek, which afterwards filled up by granulation, and the junction of that part going to form the septum with the upper lip. No attempt has been made again to approximate this; it is just sufficiently pendulous to appear natural, and no separation is observable without looking carefully underneath. The only artificial means resorted to have been the introduction of some flue of dry lint up either nostril.

In a month after the operation was performed the twisted part of the stalk was divided, and a small elliptical piece removed, to take away the projection it produced. It is remarkable how much the new part has contracted; what appeared at first a superabundant

bulging mass, is now converted into an aquiline organ of ordinary dimensions, and, as far as one can judge, corresponds as near as possible with what would have been the shape of the healthy one.

The patient is still in Sitwell's ward of St. Bartholomew's Hospital.—*Lond. Med. Gaz.*, Dec. 2, 1837.

Imbibition.—Hitherto the passage of matters into the blood has been supposed to depend on a peculiar absorbing power of the veins. But it can be shewn that fluid matters find their way without the aid of this imaginary power of absorption into the blood of the capillaries; and from the capillaries they necessarily pass first into the veins, the direction in which all the blood of the capillaries moves being from the arteries towards the veins and the heart. The primary phenomenon of the immediate absorption of substances in solution into the blood is the permeation of the animal tissues by the fluids. The property of permeability by fluids possessed by tissues even after death, depends upon their invisible porosity, and is termed imbibition. This kind of absorption being exercised by animal textures wholly devoid of life, may be correctly termed the inorganic, in contradistinction to the lymphatic absorption.

Gases, and thin fluids, together with the matters they hold in solution, permeate moist animal textures. Two kinds of gases in contact with the two surfaces of a moist animal bladder, one being within it and the other external to it, each permeate the bladder till they are equally mixed. The bladder having been previously dried and then moistened does not prevent this process taking place. A gas will permeate a moist bladder, to be absorbed by a fluid within it. This explains how it is that gaseous matters can enter into the blood during respiration, without the globules of the blood escaping. The gaseous matters permeate the membranes of the lungs, and are dissolved in the blood circulating in the numerous capillaries which traverse these membranes, by virtue of the invisible porosity of the coats of the vessels, which, nevertheless, have no openings large enough to admit the red particles of the blood. If a piece of moist bladder is tied over a bottle completely filled with water, so that the bladder is in contact with the surface of the water, and if some salt is then strewed over the outer surface of the bladder, the salt is dissolved by the water which permeates the pores of the bladder, and from this water is imparted to the water in the vessel. The primary cause of imbibition, or the permeability of animal tissues, is therefore the tendency which substances have to diffuse themselves uniformly in the fluid in which they are dissolved. A salt in solution has a tendency to diffuse itself through any other fluid with which it is miscible. Salt water and water, for example, when in contact, become uniformly mixed with each other. Animal tissues owe their softness to the watery fluids which they contain, and which fill their pores. Any matter in solution, therefore, which comes in contact with them will tend to diffuse itself in the fluids of the pores, and again, through the medium of these pores, with fluids in contact with the opposite side of the membrane, until the distribution of the mat-

ters dissolved is uniform in the two fluids which the membrane separates. There are, however, particular circumstances in which the process of imbibition is accelerated by attraction, and by the action of capillary tubes. The latter is the case when a dry animal texture is moistened, in which case the capillary attraction of the empty pores must favour the entrance of the fluid. The first case is displayed in the phenomenon of endosmose and exosmose, first discovered by Parrot, and farther investigated by Porret, Dutrochet, and others. If a solution of any salt, or of sugar, is poured into a glass tube closed below by a piece of bladder, the particles of the solution permeate the pores of the bladder, but do not pass through it. If the tube thus filled is placed in a vessel containing distilled water, the fluid gradually rises, and sometimes to the extent of several inches, within the tube, and by proper tests it is found that at the same time a portion of the solution has found its way from the interior of the tube to the water external to it. The elevation of the level of the fluid in the tube continues till the two fluids are homogeneous. If the tube contains water, and the exterior vessel the saline solution, the water sinks in the tube. If both vessels contain solutions of different salts, but of the same density, the level of the fluids does not alter, but the two salts become equally mixed. If, on the contrary, one solution is more concentrated than the other, the quantity of the more concentrated one becomes increased. The same phenomena are observed when, in place of the bladder of an animal, porous mineral substances are used. Two explanations of the phenomena have been given. The first, which was offered by Magnus and Poisson, is, that between the particles of a saline solution a compound attraction is in play, consisting of the mutual attraction of the salt and the water for each other, of the attraction between the individual particles of the water, and of that between the individual particles of the salt. This compound attraction is supposed to be more powerful than the simple attraction between the particles of water solely.* The second explanation is the following: the animal bladder, inasmuch as it is porous, may be viewed as a system of capillary tubes which exercise an attraction on the fluids, which have a tendency to mix with each other, through the medium of the water which fills the pores. If, now, it be imagined that one of the fluids is more strongly attracted by the tissues of the bladder than the other, it will, of course, be longer retained in its passage through the pores; and the level of the fluid which passes through most quickly will necessarily fall in the vessel that contains it, while the level of the former will rise until the increasing pressure of the rising column of water counterbalances the effect of the more powerful attraction.†

Dutrochet has named the phenomena which we have described "endosmose" and "exosmose," according as the quantity of the one

* Berzelius, *loc. cit.* p. 134.

† Biot, *Experimental-Physik*, translated into the German by Fechner, i. p. 384. See also Poisson, in *Poggendorf's Annal.* xi. 131. Fischer, *ibid.* 126. Magnus, *ibid.* x. 153; and Wach, *Schweigg. Journal*, p. 20.

or of the other fluid increases under different conditions. In the direct passage of matters in solution into the capillaries and the blood, endosmose without doubt takes place, and not merely simple imbibition. Dutrochet has demonstrated this by experiment. A portion of the intestine of a young fowl, half filled with a solution of gum, sugar, or common salt, and tied at both ends, was placed in a shallow vessel filled with water, when it soon became filled to distention. If, on the contrary, the intestine contained pure water, and was immersed in sugared water, it became gradually more lax, and the fluid in the intestine was afterwards found to contain sugar.*

Dutrochet's hypothesis, that electric action is connected with these phenomena, has not been confirmed. It does not also constantly happen that the denser fluid attracts more of the thinner than the latter does of the former: in the case of gases especially, the contrary is seen to be sometimes the case. But the chemical constitution of the fluid, and its physical and chemical relation to the animal membrane which it permeates, seem to have an important influence on the phenomenon. Dilute alcohol kept in bladder becomes more concentrated, the water alone evaporating;† and it has been found that if a portion of the intestine of a fowl, filled with a watery solution of acacia gum and rhabarbarin, and tied close, is laid in a vessel containing water, the intestine becomes distended, while the rhabarbarin exudes from it. Similar sacks filled with a weak solution of sulphate of iron, and laid in a solution of ferrocyanate of potash, became distended in consequence of the endosmosis of the water of the exterior solution, which at the same time acquired a blue colour from the salt of iron having passed through the membranes in an outward direction, while the absence of this colour in the fluid in the interior of the portions of gut proved that the salt of potash had not permeated them. The phenomenon of the endosmosis of gases, on which M. Faust‡ has instituted experiments, are very remarkable. A bladder half filled with atmospheric air being placed under a jar containing carbonic acid becomes more distended; and if the bladder which is placed in the carbonic acid gas contained hydrogen, it becomes distended to bursting. If, on the contrary, the jar contains the lighter, and the bladder the denser gas, the bladder becomes collapsed.—*Muller's Elements of Physiology.*

In addition to the preceding important remarks on imbibition extracted from Muller's Physiology, we beg to call the attention of our readers to the following experiments and conclusions of Dr. Rogers, published last year in the American Journal of Medical Sciences. These experiments lead to very novel and interesting

* Dutrochet, L'agent, immédiat du mouvement vital. Paris, 1826. Nouv. Rech. sur l'endosmose. Paris, 1828. [See also the article Endosmose in the Cyclopædia of Anatomy.]

† See experiments of Staples in Kastner's Arch. für Chemie, Bd. iii. II. 1—3. p. 262.

‡ Amer. Med. Journ. vol. vii. Froiep's Not. N. 646.

results, and will, we trust, be perfectly intelligible, although the text is unaccompanied by the wood-cuts which illustrate the original. The literal references to the figures we have retained, as even without the figures they make the author's meaning sufficiently clear.

On the Action of Animal and Vegetable Tissues.—Before proceeding with the subject of respiration, I propose, under the present head, to state some fundamental facts which belong to a path of research but recently entered, and hitherto little trodden, furnishing a solution to many of the most important mysteries of physiology, and shedding light where previously all was obscurity and conjecture.

The first steps in investigating the action of tissues were made by H. M. Dutrochet, to whom is due the credit of having originally called the attention of the scientific world to a new and curious class of phenomena, under the appellation of endosmosis of liquids.

Dr. J. K. Mitchell, pursuing with great ingenuity and success a somewhat similar train of research, has much enlarged the limits of the subject, by extending it to gases; and Professor Graham, of Glasgow, under the title of the diffusion of gases, has slightly adverted to the latter branch of inquiry, confining his attention more exclusively, however, to the laws which regulate the diffusion of aeriform fluids through capillary orifices.

In reviewing the labours of these experimenters, I have occasionally arrived at conclusions not in accordance with theirs, while, in many instances, I have the satisfaction of finding that our results entirely agree.

The chief and most essential point of difference between the results of the gentlemen mentioned and my own, is in reference to the view which is taken by them, that any liquid or gas invariably follows one direction, in relation to any other particular liquid or gas, without regard to the nature of the interposed structure, which my own experiments convince me does control, not only what particular gas or liquid in each case will be transmitted, but influences likewise the ratio in which they traverse the tissue. These authors would seem to conceive that the interposed substance is passive during the phenomena. It has appeared to me a point of high interest, however, to ascertain whether there does not exist an important influence due to the character of the separating structure.

My first object of attention has been to find whether or not there is a disparity in the rate of passage of different gases through different structures. For this purpose four short tubes (*Fig. 4.*) were chosen, equal in length and diameter. A portion of fresh cuticle recently separated from the *cutis vera* was tied across the end of the first (*a.*) Over the second (*b.*) was fastened a portion of peritoneum; over (*c.*) was a piece of mucous membrane; and over (*d.*) a very thin section of fresh liver. These tubes being thus prepared, and arranged over a mercurial trough, an equal measure of carbonic acid was passed up into each; a glass vessel was inverted over each of the tubes, and filled with oxygen six times in volume of the carbonic acid in each tube. The two opposite sides of the organic structures were thus in contact

with different gaseous atmospheres. A rise of the mercury in each of the tubes was soon perceived, and the rate of movement was seen to be distinctly different in each. At the end of thirty minutes the experiment was suspended, being deemed satisfactory; and the mercury in the several tubes stood nearly as represented in the figure. We here perceive that from the tube (*c*) where the mucous membrane was used, was the largest escape of the contained carbonic acid: a less proportion passed through the cuticle (*a*), a less share still through the peritoneum (*b*), and least of all through the section of liver (*d*). I should state that, while the mercury was rising in the tubes, they were depressed to maintain the same level both inside and out, in order to avoid any interference in the rate of passage from pressure. The tissues employed were as recent as could be procured. These inquiries were repeated and extended to other membranes with similar results, which I do not think it necessary to describe, as the cases given above are sufficient to establish the general proposition. As a deduction from the preceding determination, it seemed highly probable that by the use of certain tissues we might effect a separation of a particular gas from a mixture of two or more, so that by varying the tissue we might eliminate any given gas at will, performing a species of approximate analysis. To test the truth of such an inference, two tubes (*Fig. 5*) were taken, and being bent into a rectangular elbow, one extremity of each was closed by a plate of metal perforated by a small round hole, corresponding to the calibre of the tube. A membrane being placed between the plates, they were then tightly clamped together. Thus arranged, I introduced into the leg of one of the tubes, a measure of nitrous oxide, a measure of carbonic acid, a measure of oxygen, and a measure of hydrogen; and four measures of nitrogen were made to enter the other. The tissue employed in the first instance, was mucous membrane. In fifteen minutes, the mercury stood elevated in the first tube and depressed in the second; and the experiment being stopped, the contents of the latter were examined. It was found that nearly the whole augmentation of volume in this tube was due to carbonic acid. Cellular tissue was now substituted in place of the mucous membrane; and after a longer time than in the previous case, when a similar change had arisen in the volume of gas in the two tubes, the contents of that tube which previously held the nitrogen were inspected, and were found to consist of some carbonic acid, a still greater proportion of oxygen, and all the nitrogen previously present. These experiments were extended to vegetable tissues, the gases being frequently varied, and always the general results were analogous to those above in showing a diversity of action, according to the particular tissue and gases employed.

Another important branch of the inquiry, relates to the existence of a similar property in these organic structures to act on liquids. The extension of these laws of action to liquids is of the utmost importance to physiology; for nearly all the principal functions of animal and vegetable life seem intimately connected with the several specific relationships subsisting between their fluids and solids.

A bottle, filled with carbonic acid, was closed by a piece of mucous membrane, and the mouth inverted in lime-water. The marginal figure (*Fig. 6*) represents an appearance which occurred in this experiment, caused by the precipitation of the lime, as carbonate from the under surface of the membrane in a thousand minute and beautiful striæ and curls, shooting downwards, sometimes with considerable velocity, and spreading through the liquid. In this instance the membrane was slightly indented; and after ten hours the liquid occupied one-fourth of the interior of the bottle. The inner surface of the membrane was discovered to be coated with carbonate of lime, the liquid within being pure water.

A portion of the same membrane was now tied over a glass open at both ends, (*Fig. 7*;) and into the hollow formed by partially exhausting the vessel, some lime water was poured, and was found to pass through the membrane unchanged.

A bottle was filled with lime water, and a piece of peritoneum was tied over the mouth, which was then inverted into a weak solution of oxalic acid. (See *Fig. 8*.) In a few minutes white clouds were seen to form and rise within the bottle, showing that the oxalic acid was passing through into the lime water, and that the current established was only in one direction. We thus have evidence of the existence of an agency controlling the transmission of certain fluids in preference to others.

To show that the agency in question is not attributable to ordinary capillary action, and for the sake of corroborating the previous experiment, an arrangement was made consisting of six tubes (*Fig. 9*) of equal diameter, adjusted as in the accompanying figures. The tube (*a*) was drawn at one extremity to a minute capillary orifice; (*b*) was closed with a plug of dense wood; (*c*) was closed by a stucco plug $\frac{1}{8}$ of an inch in thickness; and over the lower ends of (*d*), (*e*), and (*f*), portions of peritoneum. Equal portions of a solution of chloride of sodium were put into (*a*) and (*d*); the same quantities of a solution of sulphate of potash were placed in (*b*) and (*e*); and in (*c*) and (*f*) the same portions of sulphate of copper. The glasses into which these six tubes were immersed were filled up to the same height with pure water, so that the surface of the liquids inside and outside of each tube coincided. After the lapse of six hours, the liquids in the three tubes (*a*), (*b*), and (*c*), with the several kinds of capillary termination, had all sunk through a space varying between a fourth and a third of an inch. At the same time, the liquids in the other three tubes stood raised above their former level by about an equal amount.

The disposition of the liquids was now reversed, water being put into the tubes, while fresh portions of the same solutions before employed were placed outside of them, new pieces of membrane being substituted. As the accompanying figure (*Fig. 10*) shows, the results in this case were the converse of the preceding, the change of level being about equal to the former. The time consumed in this experiment was two hours more than was occupied by the previous one.

By keeping the same liquids and varying the tissue, or by changing the tissue when the same solutions were retained, I have been able to modify these results almost at pleasure. The same tissue in different stages of disease will exert different agencies, as was manifested in the case of an inflamed peritoneum taken from an ascetic patient.

The usual or normal action of a tissue in these experiments, is disturbed by soaking the structure in some astringents. Thus, a piece of cuticle was tried after it had been dipped in a weak solution of tannin, and at another time in a strong solution of alum; and it was found that substances, which before were transmitted very readily, were now much retarded in their passage through the membrane.

Perceiving that, in many instances, the liquids performed their movements in opposition to gravity, I was curious to ascertain if they would be able to overcome a greater mechanical resistance. With this view, a piece of membrane was tied over the mouth of a bottle, (*Fig. 11*.) which was previously filled with a solution of sulphate of potash, and the neck inverted in a vessel of pure water. In six hours the membrane was found greatly distended outwards, the tension increasing for four hours longer, until the cohesion of its parts was no longer competent to balance the peculiar molecular force due to the organic structure, when the membrane burst. By an estimate which is easily made, I find that the force of transmission, in this case, could not have been less than four atmospheres, and we are entitled to conclude that it would have been still greater had the membrane withstood the pressure.

The laws of the transmission of fluids through organic structures, are exhibited in results which are equivalent to a species of chemical decomposition. To render it evident that a liquid will pass through a membrane, when the substance which it holds in solution may be retained, I instituted the following experiment.

One extremity of a tube was fitted with a membrane, as usual: a solution of nitrate of potash was placed in it, and the whole was dipped in a vessel of lime-water. After the lapse of some hours, the liquid in the tube was found elevated; and being tested for lime by oxalic acid, it gave no indication of its presence. The water had, therefore, entered the tube, but the lime had been arrested. Comparing this result with that of a former experiment, we perceive that they differ in one essential point; for, in that first performed, the lime was forced through the membrane by mechanical pressure, in company with the water, while in the other, the membrane, exerting its own influence, unaffected by any controlling power, a separation ensued.

By far the most curious experiments which I have performed with membranes are the following:

Prof. J. F. W. Johnson, of England, in his report on chemistry, made to the British Association, states that M. Wache, a German chemist, had succeeded in separating copper and one or two other metals from their solutions by the use of a diaphragm of bladder; but the rationale of the action in this experiment is not mentioned.

My own arrangements for extending the results of Wache, were suggested by this notice. One plan which I adopted, was the following:—a portion of pure water and a few fragments of iron were placed within a tube, over the end of which was a membrane, which was immersed in a solution of sulphate of copper. A deposit of pure metallic copper soon showed itself on the under surface of the membrane, covering it in two days with a beautifully brilliant botryoidal mass, thicker than a cent. The figure here given, will furnish some idea of the appearance of this deposite. By this plan I have separated gold, silver, and several other metals from their solutions. Employing the membrane in the form of a bag to contain the water and the particular metal or other body essential to the action, I have procured, by a simple method, the arbor Saturni and the arbor Dianæ under highly beautiful forms, as seen in *Fig. 14*.

In contemplating these experiments, we are led irresistibly to attribute an important office to the membrane itself. It should be mentioned, that not the least trace of the metallic precipitation shows itself upon the inner surface of the membrane, as long as this remains sound and uncorroded; and when, in course of time, some does collect there, it seems not to assume the regular and elegant form so peculiar in the deposite on the outside.

From what we see, then, to attend the normal action of a membrane, the precipitation of the metal externally, and the formation of a salt in solution on the inside, containing the same acid before in combination with that metal, we can hardly resist the conviction that the acid is liberated on the outside, passing through in its insulated state. To suppose that the sulphate of copper is transmitted undecomposed, seems to require that the copper should be precipitated on the inner side, which is not the case; and to conceive that, after being abandoned by the acid, it can retrace its way through the membrane, is to imagine a power in the structure more wonderful and incomprehensible than anything yet presented; for it implies a transmission of a body in a state in which it is considered to be undissolved. An equal difficulty attends the notion that the iron in any condition travels to the acids to precipitate the copper and return as sulphate of iron. Were the membrane clogged with the metallic deposite throughout its substance, the idea of a passage of the metallic matter might receive some support; but there is no interstitial deposite; the mass of copper, on the contrary, is readily detached, in one single piece, from the surface of the membrane, without either rupture or injury, so that it can be again employed in fresh precipitations.

Very different effects from those here detailed present themselves, when bodies of the structure denominated porous are employed. Thus, substituting a stucco-plug in place of the membrane, we find the whole of the deposite to occur on the *inside*, and none at all on the exterior surface, indicating ordinary capillary absorption.

During these experiments with membranes, I have noticed that gas is given off on both sides of the interposed structure; whereas in the ordinary chemical reaction of the same material, this evolution

of gas is not observed. The nature of the gaseous product, I have not yet had sufficient time to determine.

To bring this investigation more in contact with physiology, I performed the following experiment:—A piece of skin taken from a limb that had been amputated a moment before, was secured across the end of a tube, (*Fig. 15*,) in the manner adopted with the membranes generally, placing the cuticular surface outwards: it was carefully washed and immersed in some pure water. Some freshly drawn blood that had been kept from coagulating by agitation, was put into the tube; and after the whole had remained some hours, the water was examined, and yielded, with nitrate of silver, a distinct white precipitate, showing that a portion of chloride of sodium had been transmitted through it. Thus we see that the mere structure itself of the skin is sufficient to explain the transudation of the saline substances, which, during life, are evolved upon its surface, and, in disease, accumulate beneath it. Analogy leads me confidently to hope that secretion generally will be found to explain itself by a similar law of action, residing in all glandular structures.

Of the Evolution of Heat by Organic Tissues.—The chief changes of temperature in the organic world have been referred by chemists to friction, chemical affinities, and the numerous changes which matter undergoes in its physical forms, such as changes from the solid, liquid, and gaseous condition. I am not aware that it has been distinctly recognized or ever alluded to, that mere absorption or any molecular movement not accompanied by chemical change, has the effect of developing heat. The curious results to which I believe I have arrived upon this point, will be found, I conceive, to possess important bearings upon sundry inquiries both in physics and in physiology.

I have not been able, through want of sufficient time, to repeat and extend the interesting results which I have succeeded in procuring by the use of this apparatus, so as to put them out in the form which I consider suitable. I am induced, therefore, to withhold them for the present, in order to have it in my power to offer them at a future day in a digested shape, with the results properly tabulated.

Furnished with a number of fundamental facts derived by the several methods above explained, from direct experiment, we are now prepared to take up the consideration of the several functions which stand at the head of this essay, with a hope, may I be permitted to say, somewhat strengthened, that we are approaching a solution of their proximate causes.

It has been already noticed, in speaking of some experiments performed upon the blood, that neither exhaustion nor heat has the effect of evolving from it any carbonic acid. For the sake of seeing what effect the presence of other gases would have in promoting the separation of the gas here mentioned, five tall tubes, (*Fig. 19*,) sealed at one end, were taken, and 200 grains of venous blood passed into each. Into one (*a*) was passed 200 measures of oxygen, into (*b*) the same volume of hydrogen; into (*c*) an equal bulk of nitrous

oxide ; into (*d*) the same portion of *nitrogen* ; and into (*e*) an equal volume of *carbonic acid*. The blood in that tube, in which this fluid was in contact with oxygen, was much reddened ; it was less so in that where the nitrous oxide was ; still less where the hydrogen was ; while no apparent alteration of colour whatever was perceptible where the nitrogen was present. These conditions continued for nearly eight hours, at which time 40 measures of the oxygen had disappeared, a less proportion of nitrous oxide, a small amount of nitrogen, and no hydrogen that was discernible, but a bulk of carbonic acid, exceeding that of the blood, had disappeared from the tube (*e*.)

Agitating the several residual gases in the tubes with barytic water, not the slightest precipitate showed itself, to indicate the escape of any carbonic acid from the blood. In all those instances, where an absorption of the gas by the blood took place, there was a decided development of heat. This can be illustrated by referring to an experiment in which oxygen is employed in contact with blood in a vessel having, as seen in the figure, a cap through which a thermometer can be made to slide. The tube being partially filled with oxygen, and allowed to repose some time over the mercury, in order to acquire a settled temperature, some venous blood which had stood some time over mercury for a similar purpose, was transferred into the tube until it filled nearly all the space not occupied by the oxygen. Before the introduction of the blood, the temperature of both it and the gas was 60° ; but an absorption of oxygen very soon ensued, and after fifteen minutes the thermometer stood at 70°, when the experiment was discontinued.

A small bladder, not long taken from a pig, was filled with fresh venous blood, when it was closed and suspended by a thread from the cover of a tall receiver, which fitted air-tight. The receiver standing over mercury, was then filled with oxygen, and in two hours the mercury in the bottom of the receiver was considerably depressed. Upon inspecting the contained air, a very sensible quantity of oxygen had disappeared, but was replaced by a still larger amount of carbonic acid, the excess of which explained the depression in the mercury. This experiment was varied by making trial of other gases, as hydrogen, nitrogen, and bicarburetted hydrogen ; and in every case with a development of carbonic acid.

In order to ascertain if, during this evolution of carbonic acid and disappearance of the other gases, any elevation of temperature ensues, an apparatus, such as is seen in the margin (*Fig. 22*.) was made use of. Using my form of Sanctorio's thermometer, described above, it was plunged to the bottom in a bag, consisting of membrane, and full of blood, the bag being tightly secured at the neck, and suspended in an atmosphere of oxygen. In this instance the result was in a high degree satisfactory.

A considerably greater rise of temperature was manifested, proceeding no doubt from the joint influence of the absorption of oxygen and the transmission through the membrane of carbonic acid.

Let us now recapitulate the leading facts developed in regard to the evolution of carbonic acid from venous blood.

1. Exhaustion by means of the air-pump has no effect in evolving carbonic acid from venous blood.

2. A temperature of 212° displaces no carbonic acid from venous blood.

3. The carbonic acid, on the other hand, is *absorbed* by exposing it to venous blood.

4. Exposing venous blood to oxygen, nitrogen, hydrogen, and nitrous oxide, though each of these, if we except hydrogen, is in part absorbed, yet not a particle of carbonic acid is given off.

5. When, however, a portion of venous blood is placed in a bag of some membrane, and the whole immersed in an atmosphere of some gas, oxygen, hydrogen, nitrogen or bicarburetted hydrogen, (others have not yet been tried,) then carbonic acid is pretty freely evolved.

What a beautiful solution of the problem of respiration is presented in the facts here announced. The close accordance between the conditions of the last described experiment and those embraced in the function should be adverted to.

Among the mammalia, the lungs at each inspiration are supplied through the bronchial tubes with a large amount of atmospheric air, while the pulmonary veins, ramifying over their parietes, convey to them a large supply of blood. There, opposed to one surface of the tissue of the lungs, is an atmosphere of oxygen and nitrogen; and, in contact with the other, venous blood; so that, precisely as in the experiment, carbonic acid becomes eliminated at each expiration.

Comparing the large volume of carbonic acid expelled during every contraction of the lungs with the small quantity procured in my experiments, it might at first sight seem rather strange that so large an amount should be produced during respiration, from the cause assigned. But our surprise vanishes as we take into contemplation the disparity in the amount of surface brought into operation in the two cases. The whole area of the external surface of the bag did not exceed eight square inches, while it is computed that the mucous membrane of the lungs presents to the air a surface of not less than 1200 square feet, folded into innumerable cells, and in contact with the oxygen and the blood, between which it appears to be the means of maintaining a most curious species of connexion. Adopting the fundamental views established by the experiments described in this essay, we are furnished with a simple and satisfactory solution of the manner in which fishes and other aquatic animals having gills are enabled to separate the respirable element from the fluid in which they live.

We behold in like manner, how the whole difference in the mode of respiration of animals and plants may be the result of some simple contrariety in the intimate structure of the animal and vegetable tissues, allowing, as we see, oxygen to pass inwards and carbonic acid to go outwards, and in plants effecting just the reverse.

Whether the carbonic acid exists in the venous blood in a state of combination in the condition of carbonic acid, or whether only its

uncombined elements are present previous to its elimination as a gas, are points beyond our reach to determine.

It does not appear by any means certain that the oxygen taken into the lungs, at any one given respiration, is essential to the formation of the carbonic acid evolved immediately after ; for in some of my experiments it was found that both when carbonic acid was disengaged and when it was not, a portion of oxygen disappeared, and in other cases carbonic acid was given off when portions of hydrogen, nitrogen, and bicarburetted hydrogen were absorbed, though none of these contain any oxygen, nor can it be supposed to furnish that element in any manner to that gas. Some researches of Edwards, in which small animals were caused to breathe atmospheres of hydrogen and nitrogen, rendered the notion that oxygen is indispensable still more impossible, because, while they continued to respire at all, they expired carbonic acid ; and arresting the experiment before the animals were destroyed, it was found that each had eliminated a volume of this gas equivalent to its own bulk. I have extended these experiments to bicarburetted hydrogen, and with corresponding results. It would seem, as before intimated, that the extrication of carbonic acid is rather an action belonging to the tissue in virtue of its organized structure, so that the conjecture of Ellis, however erroneous in point of fact, which attributed to the lungs a power of secreting carbon, was an important step, approximating us to that view which recognizes in the lungs a function somewhat analogous to that of a gland.

Source of Heat in Animals and Plants.—The origin of the heat developed during animal and vegetable life, has been as much a matter of controversy as the subject of respiration itself. Any one who will be at the pains to trace all the innumerable theories and conjectures which have been invented to explain it, will find in what an unsatisfactory condition the whole doctrine rests at present.

Some have considered the chief source of animal heat to arise from the production of the carbonic acid given off during respiration. Others have imagined it to come from various chemical changes presumed to occur in the system. Some explain it by digestion ; some by circulation ; while some call in the nervous influence or innervation. If we except the first of these explanations, which has called forth much discussion on both sides, we find that the advocates of the several hypotheses enumerated, have done but little to trace animal heat to the causes which they assign. I believe I may be allowed here to say, that the important facts upon this subject, which I hope I have succeeded in developing, have hitherto entirely escaped attention. The manner of establishing the heating influence of transmission through tissues, I have already detailed ; and a still more comprehensive law of all organized structures may now be stated—that whenever a structure, in virtue of its molecular forces, causes a passage of a fluid through it, an elevation of temperature ensues. It flows as a legitimate deduction from this law, that heat must be eliminated every where throughout the animal or plant ; for, scarce a function can be executed in any part of the system that heat

will not be developed. Thus, during respiration, a tissue is traversed by oxygen entering and carbonic acid passing out from the blood. So in its round of circulation, the blood crossing from the remote extremities of the arteries to those of the veins, through an intervening mass of structure, must, in this case, also develop an increased temperature; while, in like manner, during nutrition, a similar result obtains by the introduction of nutrient matter into the circulation, and its final assimilation; for in this, as in the preceding functions, fluids are incessantly traversing the tissues of the system.

Venous Circulation.—Great difficulties are acknowledged to attend the explanation of the circulation of the blood through the veins. The principal causes assigned have been the contraction of the heart, the suction power of the heart, the contraction of the veins and arteries, and the action of the capillaries.

My own experiments proving the existence in the membranes of the body of a motive power over the liquids of the system, capable of overcoming great mechanical resistance, seem to present us with a new agency more adequate than any hitherto adduced to explain the phenomenon.

Secretion.—No direct vascular communication has been detected by anatomists between the arteries and veins of glandular structures. They trace them no farther than to the parietes of the acini; and it is not probable that any continuous connexion between the systems of vessels prevails. All the operations of organic life are executed in the minute structure of the organs. For the production of any change upon the blood, it must be subjected, therefore, to the modifying agency of this minute structure. The office of the vessels seems to be little else than that of a series of conduits distributing the fluid to appropriate parts of the system, where it is brought within the elaborating influence of the structures. We may regard, I think, every tissue as a species of glandular structure, in the proper sense of that term, inasmuch as we find that each elaborates a secretion peculiar to itself, whether it be recognized as a gland or not. For the synovial fluid is as much the peculiar secretion of the synovial membrane, and the mucous fluid as much the peculiar secretion of the mucous membrane, as bile is the appropriate secretion of the liver. The chief difference is, that, in the so called glands, the products are more apparent from being more insulated. The experiments in which substances were separated from their combinations will enable us readily to understand how this function may be performed in the system.

Nutrition.—The experimental researches detailed in the earlier part of this essay, afford us aid in explaining the manner in which nutrition is effected, equally satisfactory with that which it gives of the other functions.

We have seen that tissues possess a power of transmitting certain substances in preference to others,—in fact a separating or selecting agency; and it is interesting to notice that, very recently, Professor Daubeny has made some curious experiments upon the selecting power of plants in regard to earthy matters. We are enabled by

these facts, to understand likewise the determination of particular medicines to particular organs, a subject which opens a rich and ample field for future investigation.

As another important application of my experiments, I may allude to the light which it casts on placental circulation. The absence of any discoverable vascular communication between the maternal and foetal portions of the placenta, has seemed an insuperable difficulty in comprehending its functions; but all necessity for a vascular connexion is dispensed with, if we admit the power of a mere tissue to select and transmit liquids through its substance by an inherent force.

A further development of the facts and principles which have been presented in this essay seems destined to remove the obscurity in which the explanation of them and other functions has hitherto been involved, and to throw a broad and clear light upon most of the important phenomena of the living organization. The experiments which have been detailed form only a part of a series of investigations, by which it is hoped, ere long, to establish principles of fundamental importance in physiology, and to illustrate interesting collateral inquiries in physical science. In thus glancing at the important results which have been obtained, minute accuracy in the estimation of quantity or volume has not been attempted: but it is proposed, at a future day, to present an ample and varied detail of experiments, conducted with a view to minute precision, and the development, if possible, of the numerical laws of the phenomena in each case.—*Amer. Journal of Med. Sciences*, August, 1836.

Case of Aneurism, by J. Morrisson, M.D., Newry.—From some circumstances connected with the following case, I am induced to presume the subsequent short paper may in some degree be worthy of publicity.

Mr. C——, a travelling clerk, aged 38, of a moderately sanguine temperament, has at intervals of from one month to six, during the last five years, been subject to a series of symptoms, generally consisting of a sense of pain and weight about the loins, high coloured urine, variable in quantity, the secretion being sometimes much greater, and sometimes considerably less than usual, and frequently tinged with blood; a frequent desire of micturition; puffy ankles; nausea, and sometimes vomiting; a hard and full pulse; dry skin; headach, &c.

During this period he occasionally consulted different practitioners of eminence in Dublin, Belfast, &c., whither his avocations frequently called him, as also myself, and the disease under which he was by all invariably supposed to labour, was, that of an organic affection of the kidneys; his urine, when exposed to heat, being highly coagulable, and the *toute-en-semble* of his symptoms and appearance presenting exactly the characteristics of the disease so well known from Dr. Bright. Phlebotomy, cupping, leeching, fomentations, aperients, diaphoretics, anodynes, and rest, composed the treatment on those occasions, and which always effected a temporary immunity from every unpleasant symptom.

On the 27th of May last I was called to see him, when I found him complaining of several of the symptoms which have just been enumerated, but there were now also present great tenderness on pressure, but no appreciable fulness over the left renal region; vomiting; numbness of the left thigh; pain along the spermatic cord, and retraction of the left testicle. The pulse was full and hard; skin dry and hot; head aching; urine scanty and very high coloured. The usual indications of inflammation of the kidney being at this time so apparently evident, the treatment was regulated accordingly, and bleeding, leeching, fomentations, saline aperients, and diaphoretics were immediately had recourse to.

When I saw him the following day, the febrile symptoms had considerably subsided, but the pain on pressure over the kidney and along the course of the spermatic cord, was pretty much the same as on the day preceding.

Having the bowels now well cleared out, I made a careful manual examination over the seat of the kidneys, supposing I might detect even by touch some derangement in the structure of these organs, an organic affection having been suspected to exist in them for such a length of time. I could not however positively discover any change from the natural feel of the parts, but I regret now exceedingly I did not apply the stethoscope, as his former illnesses and his present symptoms led me at once to designate his disease as one of inflammation of the kidney.

Being anxious to ascertain whether the acute attack under which he was now labouring, had any effect on the ordinary albuminous state of his urine, I exposed this fluid to heat, when it was found fully as coagulable as at any former period.

A strictly antiphlogistic mode of treatment, with leeching and fomentations, was rigidly persevered in for six or eight days, during which time all the symptoms, with the exception of the general febrile excitement, which had greatly subsided, remained almost unchanged. Dr. Hacket, Surgeon to the Forces, was now called to see the case with me, and on finding pain in the left renal region, retraction of the left testicle, numbness of the thigh of the same side, frequent micturition, occasional vomiting, and in fact every symptom of inflammation of the kidney, without any other anomalous indication that could be detected either by touch or otherwise, fully agreed with me as to the nature of the affection.

From this period till 10th of June, no particular alteration in reference to the symptoms took place, but at this date there was an almost perfect immunity from pain, and his urine, which had continued all along high coloured and rather scanty, was now secreted in the ordinary quantity and of the usual colour.

On the 11th he felt very much in the same way as on the preceding day, and so continued till the morning of the 12th, when about nine o'clock he felt a sudden gush take place from the situation in the loin, in which was the acute pain at the commencement of the present illness. I was immediately sent for, and saw him about twenty minutes afterwards. He appeared then agitated and weak; not particularly pale; pulse 80, regular, and not very feeble; he had not

fainted. He directed my attention to the lower part of the abdomen, where on the left side a large, somewhat diffuse, and vary prominent tumour, extending longitudinally from the margin of the ribs to the pubis, and laterally from the posterior part of the lumbar region to the linea alba, was at once conspicuous. He had now great pain in the site of the tumour, and from this being supposed to be occasioned by sudden pressure, a morphine draught and warm fomentations were directed. About an hour after this Dr. Hacket saw him with me; the pain was then considerably relieved; countenance and pulse nearly natural; tumour same as before. On making a careful examination, we concluded that the tumour was caused by a sudden effusion of either blood or pus, but were by no means settled as to which, there being strong arguments in favour both of one and the other, and we were not a little puzzled in finding the tumour terminate anteriorly, exactly and definitely at the linea alba. When seen in the evening he remained nearly free from pain; pulse 86, soft; general appearance good; bowels regular.

On the 13th he continued easy, and seemed in good spirits; pulse 84, soft and regular; bowels confined since yesterday morning; got an enema which had the proper effect; tumour the same.

14th. Seems in every respect better, spirits and countenance good; free from pain; pulse 78, soft and regular; wished for leave to sit up in an adjoining room, but happily did not get permission; tumour as usual.

15th. When visited this morning about 11 o'clock, it was found he had passed a good night, and seemed as yesterday; but about 1 o'clock, P.M. Dr. Hacket and I were were hastily summoned to see him, when he described the sensation of another gush to have taken place, exactly similar to that of the former.

It was now evident from the feeble pulse, ghastly countenance, &c., that he was fast sinking; the tumour appeared somewhat larger. He lived till 5 o'clock in the evening.

On opening the abdomen the next morning an immense mass of coagulated blood immediately came into view. It filled the entire left side of this cavity; and being situated behind the peritoneum, it forced forwards and towards the right side the posterior layer of this membrane, with the intestines and kidney; and these being pressed against the anterior parietes, formed a barrier past which the effused blood could not escape to the right side; it consequently distended the left, and formed the tumour which appeared so suddenly, so prominently, and terminated so abruptly at the linea alba.

The blood, at least four quarts, being cleared away, and the intestines kept turned aside, the aneurismal tumour came into view, about the size of an orange, and arising from the left side of, and rather behind the aorta, by a large oval-shaped opening, having a thick rounded margin, exactly opposite the last dorsal vertebra. The sac was composed anteriorly of cellular tissue, and posteriorly of vertebræ, and which were perfectly denuded, and so absorbed as to form a very considerable cavity. It was lined with a dense layer of

firm, lamellated coagulum, and burst by a large opening behind the peritoneum. The kidneys were quite healthy in appearance, as were the other abdominal viscera, except from their seeming totally devoid of blood.

In making a few cursory remarks on the preceding case, I may first direct attention to the albuminous state of the urine. At different periods during the last five years, this fluid was examined by different practitioners and found coagulable by heat and acids. Now, I believe it is nearly agreed that the above state of urine designates a peculiar granulated structure of the kidneys. But here is a case, and is the only one of which I am aware, that undeniably proves, that albuminous urine may be voided, even for years, without the existence of even a resemblance of such a structure of the kidneys. Drs. Hacket and Erskine, and Mr. W. Bell, who were present at the examination, remarked that the kidneys presented a perfectly natural appearance, except that of their blanched colour. I have no doubt but Dr. Bright's statements relative to albuminous urine will generally be found correct; but I think the foregoing case will bear me out in saying, that exceptions to them will occasionally occur, and certainly it is right, and, in my opinion, not at all detracting from Dr. B., that the profession should know there are exceptions.

Whether the pressure which the aneurismal tumour, from its relative situation, must have exerted on the kidney, could have any influence in the formation of the peculiar state of the urine; or whether the juxta position in which the aneurism was placed in relation to the coeliac axis, or left extremity of the pancreas, both of these organs having been compressed, and consequently, in all likelihood, the functions of the various important parts with which they were so intimately connected, materially deranged, could have such an effect on the digestive apparatus as to cause albuminous urine, I, of course, am utterly at a loss to conjecture.

The next circumstance worthy of remark is the great quantity of blood which escaped from the circulating system, without any material impression having been made on the vital powers.

It will be recollected, that when the aneurism first gave way, a large diffuse tumour instantly appeared, completely filling the left side of the abdomen, and that the patient had not even then fainted. Now, from the size and prominence of the tumour before death, as well as from the quantity of firmly coagulated blood which was afterwards found to form it, and which, from this firmness, &c., was easily recognizable from that which had so recently been effused, there could not have been less than seven pounds given out when the aneurism first gave way. (It was not weighed, but there were three large slop-bowls full.) And when we contemplate that this quantity of the vital fluid had escaped, as it were, from the pale of the system in a moment, that it was lying in the abdomen without causing any pain, with the exception of that felt about an hour for three days, that during these three days the appetite for food and digestion had been better than for a considerable time previously, and that the patient was able to walk about his chamber free from pain, and re-

quest permission on the morning of the third day to "amuse himself in the drawing-room," this part of the history of the case will appear remarkable.

I may next observe the complete simulation of nephritis which took place at the commencement of his last illness. Whether the kidney at that time was really inflamed or not is difficult to say; but from no trace of inflammatory action having been found at the post mortem examination, I think it may be concluded, that either the pressure which the aneurism produced on the kidney, or the vertebral caries undergoing new excitement, owing to the new action in the aneurismal tumour, a powerful sympathy being now well known to exist between the spine and kidneys, was the *fons et origo* of the renal symptoms.

I shall now merely observe, in relation to the vertebral caries, that although this had extended through a great portion of the bodies of two vertebræ, no pain on pressure was experienced during life. There was no unusual sense of coldness in the lower limbs, no appreciable diminution in their power of sensibility, no involuntary twitchings, no difficulty in retaining either urine or fæces; in fact, no symptom by which it could be learned that there existed vertebral disease.

St. George's Hospital.—Abstract of a Clinical Lecture on Disease of the Kidney, delivered Oct. 10, 1837, by Sir B. C. Brodie, Bart.—Sir Benjamin Brodie commenced his course of Lectures on Clinical Surgery at the usual hour (half-past one.) He began by stating, that he would not trouble the class with any thing in the shape of an introductory lecture, as they had probably heard the subjects of such addresses already sufficiently dwelt upon elsewhere. He would only observe, that the course was necessarily limited by the number of weeks in the medical session, and that, as he was anxious to make it as useful as possible, he should select for illustration those points which were calculated to afford the greatest amount of practical information. Now these would be found to consist, not in the rarer or greater cases merely, but would also comprehend a due attention to the smaller points in surgery, which gained in importance from their frequency what the others did from their magnitude. He should chiefly connect his lectures with cases in the hospital, but would occasionally devote two or three lectures to any particular subject which he was desirous of illustrating, or which they themselves might wish to have explained to them. He wished the pupils to talk together about the cases, and to agree upon those points on which they required farther information; he would always be happy to attend, as far as possible, to their wishes in this respect; and he meant the observation to apply to the junior pupils as much as to the senior, for they had an equal right to have their interests attended to, and the hospital practice made as useful as possible to them.

He had been absent for some weeks, and had not had an opportunity of becoming acquainted with the cases; but his clerk had put

one into his hands, which seemed well calculated to be the text of some remarks.

James Lambert, ætat. 40, admitted September 20, 1837. Complaints of pain in the region of the bladder, which first came on ten weeks ago, when he likewise perceived the urine to be muddy, with a thick sediment. The pain is most severe when the urine contains the largest quantity of sediment; and it then extends to the loins and thighs. The flow of urine often stops suddenly, and this is followed by cutting pain in the urethra; after which he cannot make water for more than half an hour. Has never passed blood, but there is a white purulent deposit in the urine, which is albuminous; has no stricture.

Infus. Diosmæ, ʒiss. ter die. C. C. Lumbis, ad ʒx.

Pain in the loins was relieved by the cupping.

25th.—Urine became a little clearer, but this lasted only for a short time.

Morphiæ Acet. gr. $\frac{1}{4}$, 8vis horis. Extract. Colocynth. C. gr. v. om. nocte.

Oct. 8th.—No change.—Omitt. Morph.

The prominent symptoms here were—the frequent desire to make water, the pain referred to the penis and urethra in voiding it, and the purulent deposit in the urine. The urine, it was farther to be remarked, was acid and albuminous. Its acidity was proved by its reddening litmus paper; and the albuminous character shewn by its dropping a coagulum on the addition of nitric acid. There had been pain in the loin, and down the inside of the thigh. Now this was just such a case as used formerly to be referred to irritable bladder, but he believed it would be found to depend upon disease of the kidney. When he began practice, a patient with such symptoms used to be examined to see if he had any stricture or enlargement of the prostate gland, and it was also inquired whether he had any appearance of calculus; and if he had none of these, it was then thought that the case was sufficiently explained by calling it one of “irritable bladder.”

He had, many years ago, attended a lady who voided her urine in small quantities, with a severe pain in the urethra, &c., and he had sounded her more than once, supposing there might be a calculus in the bladder, but without finding any. None of the remedies used were of much avail, but some time afterwards she was seized with pain in the course of the ureter, which suddenly ceased, and next day she voided a mulberry calculus, from which time the symptoms were relieved. In this case there can be no doubt but that the symptoms, which were referred entirely to the bladder, depended upon the lodgment of a calculus in the kidney. He attended a gentleman whose bladder could not retain above a table-spoonful of water; the urine was albuminous and purulent. No stricture of the urethra existed, and there was no stone to be found in the bladder. In this case, too, the remedies proved inefficient: at last he was seized with pain in the course of the ureter, extending to the bladder, and followed by the expulsion of a solid portion of lymph, with

a fimbriated extremity. He had several similar attacks, and under one of these he died in the country.

Morgagni relates a case of this kind.—A boy had pain referred to the urethra and bladder, with inability to retain his urine; he died, and on examination after death, the urethra, prostate, and bladder, were found to be healthy, but there was a stone in the kidney. Sir Benjamin had himself seen several cases more or less similar, that is to say, in which symptoms referred to the bladder had been proved to depend upon disease in the kidney. He had attended a patient who laboured under frequent desire to make water, pain in the urethra and neck of the bladder, and albuminous urine, and who ultimately died with disease in the kidneys, and none in the bladder.

In another case the patient had similar symptoms, and died from a cause foreign to urinary disease, namely, from bursting of the gall-bladder, and subsequent peritoneal inflammation, for which he was attended by Dr. James Johnson. Now here there were found a dark-coloured and spongy condition of the kidney, with deposits under its external coat of bodies of transparent appearance, resembling hydatids, though essentially different in character, while the tunic of the kidney adhered so firmly to the fat of the loins as to remain *in situ*, while the kidney was peeled out from it. The bladder was free from disease, although all the symptoms had been referred to it and the urethra.

But it is only when patients labouring under disease of the kidneys die early, cut off, as it were, by some accidental occurrence, that the dependence of the symptoms exclusively on such affection of the kidney can be proved, because disease of one part of the urinary system soon brings on disease of other parts. One of the first of such extensions is to the bladder, the mucous membrane of which becomes irritated by the morbid state of the urine, and then inflamed; being once inflamed, it in its turn secretes a ropy, tenacious mucus, which sticks like bird-lime to the interior of the vessel into which it is voided. Portions of phosphate of lime frequently show themselves; and now the urine becomes alkaline, and is often extremely foetid.

But another and peculiar circumstance which sometimes attends disease originating in the kidney, is abscess at the neck of the bladder. Sir Benjamin has seen several examples of this; *i. e.* of patients having disease of the kidneys, whether suppuration, calculus, or other change, becoming affected with abscess at the neck of the bladder, and in one such he had opened the abscess in the perineum, and evacuated a large quantity of pus, the probe passing into an extensive ulcerated cavity connected with the prostate gland. Perhaps this phenomenon was explicable on the principle of the nerves of the part being in a constant state of irritation—a condition which, under other circumstances, was productive of inflammation and its consequences. Thus he remembered that in the case of the late Dr. Pemberton the face was swollen on the side where he had suffered most from pain. Now his disease was an aggravated instance of the *tic douloureux*, and Sir Benjamin inferred that the excited state of

the nerves had, after a certain time, led to inflammation in the cellular tissue of the parts. So, likewise, with regard to abscess which formed at the neck of the bladder in disease of the kidney. The nerves were then in a state of constant irritation, as indicated by the inability to retain the urine, and this in some constitutions, was productive of inflammatory action on the contiguous parts, ending in suppuration.

With regard to the kind of disease which took place in the kidney in such cases as he was considering, Sir Benjamin did not think that it was necessarily always the same. Sometimes it seemed to be a form of chronic inflammation, either limited to the tubular part of the gland, or pervading its whole structure: sometimes there appeared to be distinct abscess formed; at others the pus would rather appear to be formed by the lining membrane, without any ulceration. Sometimes there was calculus impacted in the kidney; sometimes there were those serous cysts to which he had already alluded; and sometimes the gland was altogether converted into a collection of cysts or vesicles, the glandular structure being nearly or wholly destroyed.

The symptoms which characterized disease of the kidney at its early stage, such as the case before them, were pain referred to the penis and urethra, with incontinence of urine, so that the patient is obliged to void it sometimes so frequently as every half hour; uneasiness, sometimes amounting to pain, in the region of the bladder itself; and occasionally, but not necessarily, pain in the loins, or in one loin, corresponding to the diseased kidney. The urine at this period is sometimes quite clear and healthy, at least it is at times free from albumen, but always becomes opalescent sooner or later, and coagulates on testing it with nitric acid.

To these symptoms at a more advanced stage become added paroxysms of pain, extending from the kidney to the bladder, with affection of the testicle or inside of the thigh. The urine also contains pus; and when this comes suddenly in considerable quantity, and afterwards nearly disappears, it may be conjectured that it is from an abscess. But in other cases pus continues long to show itself as a deposit in the urine, without the symptoms of abscess, and is probably secreted by the kidney without abrasion of surface. Frequently, as in the case before them, there was a deposit of purulent matter; and whether this were so or not, it was acid and albuminous; not that it invariably had these last characters, but they were always present at times in the course of the disease.

Then the urine is usually possessed of another and very peculiar character in its smell. No peculiarity of this kind admits of description; but of the familiar odours which it most resembled, that of musk came nearest. Now this musk-like odour must not be confounded with that which arises from mucus secreted in the bladder; this last is ammoniacal, and the urine often absolutely stinks. This latter state is owing to inflammation of the mucous membrane of the bladder, and the decomposition of its secretions, so as to evolve ammonia.

With respect to treatment: one of the most distressing circum-

stances was the inability to retain the urine, from the irritability of the bladder, which, as already stated, was sometimes intolerant of half an ounce of that fluid. Now something might be done to relieve both this and the pain accompanying it, by the use of opiates; but he was, upon the whole, averse to these, as they sooner or later interfered with the functions of the stomach, which it was of great importance in these cases to cherish: besides which, the opium had no tendency to cure the disease eventually. He therefore limited his patients to a suppository of opium introduced into the rectum, or a clyster at night. In regard to the power of the bladder, he had known a patient improve this by accustoming it to the presence of quantities gradually increased: he had injected two ounces of tepid water, and, persuading the bladder to retain that, had increased the injection to half a pint; thus, by perseverance, enabling the bladder to acquire better habits, so that he was able to go into society, which he had previously been prevented from doing. But those two, (the opiates and injections,) where it was expedient to try the latter, could only be looked upon as palliative means. Considering the symptoms during life, and the result of post-mortem examinations, there was reason to regard such cases as very generally connected with inflammation about the kidney; and in keeping with this, was the fact that he had seen considerable and permanent relief from counter-irritation being established in the loins, either by means of a seton or caustic issue. He had also seen blistering of much service.

As to internal remedies more particularly directed against the disease, he thought that a distinction was to be made between the condition of the bladder often consequent upon disease of the kidney, and original disease of the latter. The former, when attended with secretion of ropy, alkaline mucus, was frequently much benefited by the Pareira brava; but this had little, if any effect, on the kidney. In the case before them the decoction of the Diosma had been ordered, in doses of \tilde{z} jss. three times a-day. This was a remedy from which he had repeatedly seen unequivocal benefit in such cases; and another, which also occasionally proved of service, was the Uva ursi, but for this purpose it required to be given in much larger doses than those usually administered; perhaps the best form was a decoction of \tilde{z} ss. or \tilde{z} j. in 6 oz. of water. There was a popular medicine, too, which he would mention, though it had not found its way into the Pharmacopœia: he meant the seeds of the wild carrot. This plant, which had long been used for such purpose among the people, was always kept at Covent Garden, though not to be had at the druggists.

The hour having expired, prevented Sir Benjamin from concluding the subject in the present lecture.

Military Hospitals during the Siege of Oporto.—We have extracted the following from the Personal Memoirs of Colonel Charles Shaw, a most interesting work, lately published in London. Gentlemen who intend to become military surgeons may learn from these extracts the general nature of the arduous duty they may be called to perform when engaged in actual service.

The duty which now had to be performed by the medical men was of the most arduous character. The surgeon of the British, Souper, carried away by the military spirit instilled into him by being an actor in the three days of July, resigned his commission as surgeon, and on this day commenced and finished his military career, being killed at Hodges' side while carrying orders to the French battalion; his place was filled up by Mr. Rutherford Alcock, who had the same love for "fire," but for a different object—that of being close at hand to give prompt assistance to any who were wounded. Although young, Alcock was old in knowledge and experience. He was highly respected by all who knew him, and beloved by those who entered into action, as they felt assured that he thought not of his own safety when his services could be of benefit to them. In the most exposed situations, I saw him this day dressing officers and men with the same coolness as if he were in a London hospital. And I cannot refrain from expressing envy at the gratified feelings he must ever possess, when he thinks of the number of human beings he has saved by his knowledge, experience, bravery, and activity, both at Oporto, Vittoria, and St. Sebastian; but his trials after the fight of the 29th of September were great.

Owing to the fights of Pennafiel, Ponte Ferreira, and the different affairs on the Lugar das Antas, the wards allotted to the British in the general hospitals were full; therefore one may form some idea of the misery of the British when scattered among the different hospitals, speaking a language which was not understood. Measures were taken by Hodges and Alcock, to gather the wounded foreigners together; but the minister of war threw every impediment in the way of this—almost making one suspect, *that now that the soldier had done his work, and was useless, the sooner he died the better*. Truth compels me to state a fact I should wish to avoid; but it is right that those who are to be soldiers should know the value that is sometimes put on their services. The words were made use of by Don Pedro; but from what I have seen of him, I think others must have at the moment prompted him. The medical man was mentioning that it would be necessary to amputate the legs and arms of some of the British. "*No, no,*" said Don Pedro; "*you British are fond of amputations, because your men are to have pensions, and that is expensive.*"

No application from myself as commanding the battalion; from Alcock, as senior medical officer; nor from Hodges, as the representative of the foreigners, had any effect on Augustinho Jose Freire. Thus the poor fellows, crowded together, without beds, without nurses, without clothes, and even without medicines, died in numbers. A young man who had joined me some days before the 29th, as a volunteer, had been reported to me as killed; indeed I had viewed a mangled body which I was told was his. I visited the hospital daily, and about the middle of October, in passing through a ward, I thought I distinguished a voice from a corner, repeating my name. I looked, and there discovered on the bare boards poor —, in a state of nudity, imperfectly covered by a dirty blanket,

filled with vermin. Before this he had been too ill to speak. If I had taken an active interest in this young man, all the other wounded would have been offended; but I sent my servant to pretend he was an old friend of his, and thus gradually supplied him; and I am happy to say he recovered, and is now a thriving man in India.

How little do young men know of war, and all its miseries! I do not wish to disgust young fellows with the military profession, as, with all its drawbacks, I prefer it to any other; but how apt a young man is to be led away, when he sees an officer at home without his arm, to say to himself, "how I should wish to look like that officer." He forgets the starvation endured before going into action, the cold and bitter nights spent in drenched clothes, in wet fields in bivouac; the momentary forgetfulness of all misery in action, until the shoulder bone, by a shot, is splintered into bits; then the little sympathy felt, every one being for himself; then the excruciating pain endured by the shaking of the bullock waggon, or the want of care in carrying him away; the little bones coming through the skin, making him shrink with agony; then the time he is allowed to lie on the cold floor of a church, until the surgeon comes to dress him in turn; then the pain of amputation, and when that is over, the necessity of shutting his ears to the screams of the dying, and his eyes to the corpses of those carried past him, who, a few minutes before, had suffered an operation similar to his own. This appears to him very shocking, but this is nothing compared to the disgust which he experiences in the dressings, washings, splinterings, bandagings, and cuttings out, which are the daily, nay hourly detail, of military surgery. Of the foul air caused by so many confined in the same spot, and suffering the same inconvenience; some idea may be formed, but no description can be given. Worst of all, too, the patient is obliged to witness the deaths of many around him, who, almost before the breath is out of their bodies, are robbed, and have their effects distributed among the attendants, most of whom volunteer this service to have an opportunity of plundering the dead and dying.

Often when a patient is thirsty, these attendants are either too hardened or too drunk to be able to give them a drink, and very possibly offer him the nearest liquid to them, probably something intended for a wash, or while the expiring man is saying his prayers, a wretch is holding up his head with one hand, while he is stealing the dollars of the dying man with the other. This was the daily scene for many days in the hospitals at Oporto after the 29th, until they were to a certain degree emptied by death. With some of the officers it was better, but still, take away the romance and let the truth appear, and who would eagerly embrace the military profession?

A handsome young fellow is lying in a comfortable bed, he has a severe wound in the leg, this you do not see, but come into the room where the surgeon is dressing the wound, and you are driven out by the unsupportable stench. Alas! instead of taking an interest in, you have almost a feeling of disgust for the sufferer.

I could dwell on this subject at great length, but I refrain. Offi-

pers who have witnessed these sights must allow I have not exaggerated them.

Effects of Water applied to the Skin in allaying Thirst.—The heat was dreadful, and the enemy had cut the ropes of the different wells. I had learned from my walking experience, that to thirsty men drinking water only gives a momentary relief, but that if the legs are wetted, the relief, though not at first apparent, positively destroys the pain of thirst. Seeing a muddy pool at the bottom of one of the hills, by which we must pass to attack the convent of Bostillo, I halted for a few minutes, making the men wet themselves from the knees downwards.—*Ibid.*

How to stop a Bleeding from the Nose.—A very nice-looking young girl was seized with a severe bleeding of the nose, which neither cold keys nor links of chain cable put down her back would stop. It became so alarming that her mother called out for a medical man, but not one was on board. However, I undertook to complete the cure, provided I was allowed to follow my own plan. The mother would not hear of it, because I was not a medical man; but the young lady said she would allow me to do as I liked. On this I took her to the back cabin, began to loosen the back part of her gown and her stays, which were tightly laced. I then called for sea water, and, having dipped a towel in it, put it quite wet around her neck. I then took another towel, and having also dipped it in the water, squeezed it, and allowed the water to trickle down her back, which soon made her creep together, and the bleeding stopped. The cure, though effectual, was not pleasant, as it left the poor girl's gown and clothes in a sad plight; but I got thanks, and a kind invitation from the young lady to visit them in Guernsey.—*Ibid.*

Medical Directions for the Use of Pedestrian Travellers.—If any one intends to make a long tour on foot, it is necessary to take some precautions. I need not say that English shoes are the best. I do not mean new shoes, but those of which the upper leathers are good and soft, and have been worn to fit the shape of the foot. To such a pair of shoes let an additional sole be put, with small nails at the toes and sides, care being taken that the heel be not either too high or heavy. Let them be laced a short way up the instep, and of a size to allow the foot to sit easy without being loose, when a woollen stocking is on. Of these strong shoes have two pair, and a third pair, not of such strong material, to be worn when you come to the end of your journey. As to the stockings the greatest care must be taken in the choice, as such as are generally sold in shops are sure to cause blisters both at heel and toe. If you examine the ordinary qualities of stockings in shops, you will find that the threads are drawn together to a point in the middle of the heel, and about the ball of the big toe. Avoid such stockings, as they are sure to cause misery. The stockings made by old women on wires are the best, and the finer the wool the better. Of these there should be four pairs; and if a stock-

ing be put over each shoe, (the outside innermost,) they will not take much room, and will at the same time prevent the shoes from soiling the other things in the knapsack. As to other requisites, the first to be provided is a good knapsack of the best oil skin. It is to be had in all the military store shops in London. Care should be had to have the straps of the best patent leather, and a degree broader than usual. The proper breadth for ease is the regulation strap for the Guards' knapsack. They should be so long that you can use them in the foreign manner if you choose. By this I mean, that in the foreign knapsack the fixture from which the shoulder straps play, is placed in the centre of the knapsack, while the English fixtures are placed on the points of the shoulders, just in a line with the shoulder straps, so that the whole weight of the knapsack is on the upper part of the arms, instead of being divided over the back. In the French manner the knapsack sticks closer to the back, consequently you do not feel its weight so oppressive.

When provided with a knapsack, get a wide cloak, (so wide as to go over the knapsack,) of the very finest silk oil skin, long enough to reach to the middle of the thigh; likewise an oil skin to the hat. Caps are recommended, but a hat is preferable, as you can carry things in the hollow of it. For a coat nothing is so good as a surtout made of the finest cloth; it should button up close to the neck to avoid cold: the oilskin cloak can be used either for sitting or laying on the ground. Have two pairs of trowsers of dark gambroon. As it is of consequence to walk cool, if possible, march without drawers, but be sure to put them on at the end of the journey; one pair is enough; they can be washed and dried while you are in bed. As to shirts, have one in the knapsack, and a very long night shirt made of the finest and lightest cotton, which will be found of the greatest benefit, when you are not sure of the cleanliness of the bed. If your trowsers are wide you can even wear it at the end of a day's journey. Of course a fresh flannel under-vest must always be in the knapsack. The best gaiters to wear, are those used by the French when shooting. They are made of the strongest soft leather, with straps to tighten, if necessary. They should be as high as the knee, and buckled over the trowsers, so that, however dirty the roads may be, on throwing them off, you find your trowsers quite clean and dry. The great difficulty in walking is to keep the feet in good order. This can be done if a little attention be paid at first. For some days before starting dip your feet in hot water as often as possible for a few moments, and then rub them quite dry. Let this be done morning and evening, till you find the feet quite free from a damp feeling. Provide yourself with a good sized tin box, full of the best yellow, or, as it is called in some places, soft soap. It has something the appearance of honey in the comb. Before starting in the morning, rub the soles of the feet, especially about the heels and toes, with the soap, until it has the appearance of a good lather for shaving, and then put your woollen stockings on. Let this be done every morning before starting, and you will find even in the hottest or wettest weather you will be able to do a great deal of work, and

at the end of the day find your feet cool and free from blisters. Instead of washing the feet at the end of a journey, rub them first with a damp cloth, and then dry them completely. In some places on the Continent it is not possible to get this soap; but in almost every apothecary's shop you can purchase stag fat, which does very well; and if you cannot get stag fat, buy goose fat or hog's lard. With these fats, I first rubbed the feet with spirits, which is an improvement; but nothing can stand comparison with yellow soap. Have your stockings washed as often as possible; and if they have not time to dry during the night, they can be easily buckled on the outside of the knapsack. By attending to these directions, and by instantly rubbing yourself dry, and putting on fresh flannels and linen at the end of your day's work, and eating as much animal food as possible, yet drinking no more than is necessary, both body and feet will get into the highest condition.

To prevent thirst in hot weather, nothing is better than to take a great quantity of fresh butter with your bread for breakfast. Avoid drinking water as you would poison; in short, drink as little as possible of any thing, and do not give way to the first sensation of thirst. I strongly recommend starting at day break, having previously taken breakfast.

[Colonel Shaw had frequent opportunities of observing the effects produced by long-continued want of sleep and fatigue, and he has made the very interesting remark, that the human body is thereby reduced to a state not easily to be distinguished from drunkenness.]

I was obliged to flog some for sleeping on sentry, and for being drunk on duty. Nothing is so difficult, in my opinion, as for a court in certain cases to decide on drunkenness. When duty has been very severe, and when officers and men have for some time had little sleep, I say distinctly, that no man can distinguish between the effects produced by continued want of sleep, and the effects of liquor; and I never did flog a man (when duty was severe) for being drunk on duty, unless there was direct evidence of the accused having been seen to drink liquor.

Many is the active good soldier I have seen confined for being drunk on duty, when this was owing to fatigue and want of sleep.—*Ibid.*

On Injuries of the Eye by Percussions Caps, by Samuel Crompton, Esq.—The following observations were made in the practice of Mr. Barton, Surgeon to the Manchester Eye Institution, to whom I am indebted for permission to publish the illustrative cases, and his method of treating them.

One of the fragments into which a percussion cap breaks when it is exploded, sometimes enters the eye. The accident generally occurs in shooting with, or in discharging, percussion caps with a hammer. I have seen many instances of it, and have preserved notes of seven cases, in each of which the injured eye was destroyed. In one of these cases the vision of the other eye, also, was nearly lost, from sympathetic inflammation; and it is most likely that there would have been a similar termination of the rest, if that treatment,

which I shall presently describe, had not been adopted. The peculiarities and importance of these injuries, and the consideration that they are unnoticed by systematic writers on the eye, have induced me to detail, more minutely than would have otherwise seemed necessary, the most remarkable circumstances relating to them.

In every case the fragment of cap was driven into the posterior chamber of the eye ; but immediately after the accident, the changes produced in the eye, and the symptoms, were so like those observable in penetrating wounds of that organ, when no foreign body remains in it, that it was impossible to ascertain, at first, whether the cap was in the eye or not. The wound made by the entrance of the fragment of cap into the eyeball was generally a clean incised one, and healed readily. The vision was not destroyed immediately in those cases in which the cap went through the sclerotica, and did not injure the transparent parts of the eye. For a length of time, varying from a few days to a month after the accident, the patients appeared to be in a fair way for immediate recovery ; but at the expiration of that time they were suddenly seized with most acute pain in the eye, attended with extensive chemosis, and with haziness of the cornea in some of the cases. Suppuration never happened. The pain subsided entirely for a while, or was greatly mitigated in a day or two after its commencement ; but this cessation was only temporary, for it always recurred and subsided at uncertain periods, until the vision with the injured eye was entirely destroyed, the eyeball in a state of painful chronic inflammation, and the health of the patients much injured by the irritation occasioned by the injury, and from anxiety for their sight ; for the vision of the other eye became affected at this stage of the disease, by the inflammation extending to it by sympathy. The first indications of its commencement there were a slight redness of the conjunctiva, and an inability to see so well as formerly with the eye, or to bear the ordinary light of a room without pain and confusion of vision.

In Case I. the effects of the sympathetic inflammation were, a dull yellow colour of the sclerotica ; a change in the colour of the iris, and adhesion of it to the capsule of the lens ; and a very irregular and small pupil, filled with a dot of opaque capsule.

The fragments of caps taken from the eye, after being within it for months, were only tarnished ; they bore no appearances of undergoing changes similar to those which take place in pieces of steel during their exposure to the humours of the eye ; they were always of a considerable size, and their angles were very sharp. Mr. Barton believes that the sympathetic inflammation in these cases is occasioned by the presence of a fragment of cap in the other eye, and that the only means of preventing it, or of allaying it when it has arisen, is the removal of that fragment from the eye. He has treated many cases on this principle ; of seven of which I have preserved the following notes.

CASE I.—W. —, Esq., about 40 years of age, and of very intemperate habits, was shooting on the moors in August, 1832,

when, on discharging his gun, something cut his right eye. He lived far from Manchester, and was under the care of his usual medical attendant, who used active measures to subdue the pain and inflammation which occurred in a few days after the accident. He consulted Mr. Barton, for the first time on the 29th October, 1833, when he had continued pain in his right eye, occasionally so severe as to prevent him sleeping for successive nights; his vision with it was destroyed; the left eye also was inflamed sympathetically, and vision with it so much impaired that he could not find his way. His health had suffered greatly from the effects of the disease, and his anxiety for the recovery of his sight. Mr. Barton told him that it was very probable that something had entered the eye and occasioned his sufferings; but, in compliance with the request of the patient, who was unwilling to submit to an operation for its removal, various plans of medical treatment were tried until the 3rd of November, without the least benefit being derived from them. On this day a large piece of the cornea of the left eye was cut away, in order to remove the foreign body; but the eye was so exquisitely sensitive, that attempts were not made to find it. A large poultice was applied to the lids. In a few days after a large fragment of a percussion cap was removed from the coagulum, which filled up the opening that had been made in the globe of the eye; it was merely tarnished, and its angles and margins were as sharp as if it had been just broken. The patient was permanently relieved, but the sympathetic inflammation had produced so great changes in the other eye, that it was necessary to perform an operation for artificial pupil upon it.

CASE II.—George Ankers, of Staly-Bridge, about twenty-eight years of age, on the 29th January, was sitting near a man who discharged a gun, when a portion of cap entered his right eye. On the following day he could see across a room with that eye, but in the course of a month vision with it was quite lost. On the 8th April the conjunctiva was very vascular, and the eye occasionally very painful. He could not bear the ordinary light of a room without placing his hand over the other eye (the left), neither could he read more than one, two, or three lines with it, before the letters became indistinct, and the eye painful. A flap of the cornea of the right eye was removed, and a poultice was applied to the eye-lids. On the 16th of June, a friend removed the fragment of cap from the cicatrix in the front of the eye-ball. He has been easy since, and the powers of the other eye are quite restored.

CASE III.—Master R., of Stockport, about six years of age, was playing, on the 25th of July, 1836, with a boy who was exploding percussion caps with a hammer, when a fragment of one cut his eye. This eye was so free from pain and inflammation for several weeks, that it was hoped that the cap had not entered it; but by the 21st of September it had assumed the appearances indicative of its presence there. A portion of the front of the eye-ball was cut away. On the following morning the fragment of cap was found in the

poultice which had been applied to the eye-lids ; it had only become of a darker colour by being in the eye.

The following cases were treated as the above :

CASE IV.—W. Williamson, of Stockport, injured in shooting.

CASE V.—Adam Chamley, of Hebden Bridge, Yorkshire, injured in shooting.

CASE VI.—Mr. T., of Cumberland, injured in shooting.

CASE VII.—John Taylor, of Manchester, injured whilst standing near a man exploding a percussion cap with a hammer.

The details of the four last cases are so like those of the three first, that it seems unnecessary to give them. In all, however, the object of the operation was gained ; the sympathetic inflammation being suspended in the first case, and the symptoms which were thought to indicate its approach being removed in the others. The operation is thus performed :—The patient being placed in a convenient position, the operator forms, by means of Beer's knife, a large flap of the cornea, which he seizes with the forceps and cuts away with a pair of curved scissors. A dose of laudanum is then administered to the patient, and a linseed-meal poultice applied to his eye-lids. The operation always gives great pain, and should be performed as rapidly as possible. The eye is so exceedingly sensitive, that attempts to find the fragment of cap cannot be endured. In all the cases of Mr. Barton, the cap was found in the poultice, or in the coagulum which closed the opening into the eye, in a day or two, or at a longer period after the operation.

I have made diligent inquiries as to whether the caps which inflicted the injury were grooved or smooth. I believe that they were generally smooth and of an inferior kind, called French caps ; but it is very likely that both kinds are very dangerous when exploded between two flat surfaces on a level with the eye ; an amusement with children which is very common in this part of the kingdom.—*Medical Gazette*, Oct. 28th, 1837.

Puncturation in Ascites.—SIR, The patient treated for ascites by puncturation, of whom there appeared some account in the number of your Journal of the 7th ultimo, has continued to improve in health, and is now nearly well. That account extends to the 25th of September last, at which period I had made thirty-eight punctures, and obtained a reduction of four inches in the circumference of the abdomen, with alleviation of the urgent symptoms, and marked amendment of the general health of the patient. Thirty-three punctures have been made since that time ; in September, two on the 26th, two on the 29th, and five on the 30th ; in October, two on the 2nd, two on the 4th, two on the 5th, one on the 10th, one on the 11th, one on the 13th, one on the 16th, one on the 18th, two on the 20th, two on the 22nd, one on the 24th, two on the 26th, and three on the 30th ; in November, one on the 3rd, one on the 6th, one on the 11th, and one this day. Their mode of action appears to be this : the fluid, with the exception of a small quantity which escapes through the opening in the skin, oozes for three or four

hours from the cavity of the peritoneum into the subcutaneous cellular tissue, which it loads and cedematizes. From hence it is gradually absorbed, and chiefly during the next five or six hours; after which the patient voids a good deal of urine, and often perspires pretty freely. This process appears to be continued or repeated more or less completely, until the peritoneal orifices of the punctures close. Thus has the patient been relieved of the greater part of the fluid which the peritoneum contained. He is now almost restored to health. The circumference of his abdomen is now only three feet four inches, being one foot, four inches less than when the operation was first performed; his pulse is at 72; his breathing free; the muscles have regained firmness and strength, and he is beginning to resume his occupations as a surveyor. His tongue is still rather red at its margin, and smooth on the surface, but the appetite is good, and the stools are natural. The urine appears to be of good quality; it is frequently abundant, and then rather pale and aqueous.

The punctures were performed nearly on a level with the umbilicus, with the exception of eight or ten made three or four inches above it: about one-third of them were made in the linea alba, and the others on either side, and within a distance of this line varying from four to eight inches. With regard to their number and the days on which they were practised, I was guided somewhat by the effect they produced on the disease and on the patient. As they sometimes left a little soreness, and I found latterly a small number answer as well as more, I made the fewest requisite. The soreness, and occasionally the indisposition of the patient, sometimes induced me to make them less frequently than every other day, which, however, upon the whole, seemed to me to leave the fittest intervening time from one operation to another.

During this treatment the diuretics and blue pill previously administered by Mr. L'Estrange and myself, have been discontinued. We have given only potas. nitrat. gr. xii. thrice a day, with whey and parsley tea.

I remain, Sir, your obedient Servant,

THOMAS KING.

Ibid.—Nov. 25th, 1837.

On Pessaries, and the Radical Cure of Prolapsus Vaginæ et Uteri, by Professor Dieffenbach.—This distinguished surgeon has long discontinued the use of pessaries in his own practice. To them he ascribes the occurrence of many diseases of the vagina and uterus, as well as of the neighbouring parts; and although he admits that there may be cases in which their use is likely to be beneficial, he considers that such cases are comparatively very rare. He was led to adopt the mode of practice which he here recommends, by seeing the case of a woman, the subject of prolapsus of the vagina and uterus, in whom parts of the vagina sloughed, during its state of prolapse: the uterus and vagina were replaced whilst granulation was going on, and the result was a complete cure of the disease. The first case with which Dieffenbach met, after this, on which he was

determined to imitate the natural process, was that of a woman with prolapsus of the uterus, which could be easily replaced, but as easily prolapsed, when it was not kept in by a sponge.

The operation was thus performed. The bladder and rectum were emptied; the uterus was made to prolapse, and a portion of about the size and shape of a hen's egg was removed from the left side of the vagina, the sharper end of which was directed backwards, the opposite end forwards, and came in contact with the nymphæ. The fold was then seized with a pair of forceps, the uterus being previously pressed somewhat backwards to take off the tension of the vagina, and then dissected out with a slightly curved scalpel. The same process was repeated on the right side. The wound was cleansed, and at its hinder part two sutures were applied, the uterus was next replaced, and three other sutures were applied within the vagina. Had all the sutures been completed before the attempt was made to replace the uterus, it is possible that its reduction could not have been effected. Some little irritation followed, which ceased, however, on the removal of two of the sutures from either side. On the sixth day, all the sutures had separated.

Since the time at which Dieffenbach performed this operation, he has repeated it very often. He now employs a smaller number of sutures, usually only two, and never more than three. In many cases he uses no sutures at all, as the borders of the wound in the vagina mostly lie low in contact after the uterus has been replaced. The suture is required where there is great relaxation, and a want of irritability of the vaginal membrane; on the other hand, when the individual is robust and the vagina thick, it is better to dispense with sutures. When the surface of the vagina is mortified, it is necessary to fill it with charpie. Tepid mucilaginous injections should be used for some days, and after these, cold water. If, when cicatrization is going on, there is no evident narrowing of the vagina, a compress of charpie, smeared with a resinous ointment, and the repeated application of the lapis infernalis, should be employed.

Dieffenbach has often removed the fold from the vagina after having replaced the uterus, by drawing a portion of the former outwards, and cutting it off by a knife with a sawing motion. This is a far easier mode of operating, but great care is necessary not to injure the bladder or rectum, which may happen if the fold of vagina, when tightly stretched by the forceps, should be cut of too near its base. Sutures are not employed in this case.

The position of the patient, in the operation above described, should be the same as that for lithotomy. The state and relations of the rectum and bladder with the vagina and uterus should be ascertained, previous to the operation of the former, by means of the finger, of the latter, by Desault's silver catheter. The catheter sometimes draws off a quantity of retained urine; the evacuation of the bladder being often rendered very difficult by the prolapse of the uterus.—*Medicinische Zeitung*, No. 31. 1836.

On Malaria and Yellow Fever in the West Indies, by Dr. William Ferguson, Inspector-General of Army Hospitals.—In my last letter I endeavoured to point out the localities and characteristic signs of the presence of malaria—its qualities, extent, and boundaries; but on reflection it has struck me that some points of the inquiry were not sufficiently elucidated, and to them I shall now refer. I have stated that the low levels of country near that of the sea are the abode of its worst product—the true yellow fever; and it must be interesting to every military reader to be informed how far above them that form of the epidemic is to be dreaded; and here we have a case in point.

In the year 1816 a fatal yellow fever pervaded the Island of Antigua. The epidemic influence was all but universal, and even the negroes became susceptible of its lesser grades, suffering from the intermittent and remittent types. Under these circumstances the swamps surrounding the dock-yard of the English harbour became most deadly, and healthy white soldiers taking the night guards there would be seized shortly after standing sentry, and expire in all the horrors of the black vomit, within a short time after being carried to their quarters on Monk's Hill, immediately behind and overhanging these very swamps. It is an insulated, fortified hill, 600 feet high; but not a case of yellow fever arose there during the whole course of the epidemic; that is to say, the staff, the non-combatants, the families of the garrison; all, in fact, who slept regularly within its walls remained free, while the soldiers, who took the night guards below, were cut off in the manner I have stated: but we must not from this conclude, that although a clear unscreened elevation of 600 feet would confer security, the same rule is to hold good in regard to the sheltered niche or terrace of a mountain ridge above the marsh, which in numberless instances has been proved to be more pestiferous, at even considerable elevations, than the marsh itself. We can always, where there are high mountains, in the course of ascent, rise above the malaria, and it is probable that none, even of the mildest, can be felt at a greater elevation than 1800 or 2000 feet above the level of the sea; but at less than 1500 feet, wherever the country is jungly and impervious to the breeze, its presence, however modified, is always to be dreaded in some shape or other.

In the lower levels such jungles often become incredibly pestiferous. I have even been inclined to doubt, when I contemplated the unbroken silence that reigns in them, whether the poison might not also be destructive of animal life—I mean the warm-blooded animals; for no bird flits there, and no quadruped is to be seen; or whether the serpent of the woods, by destroying equally the young of bird and beast, may not have acquired the sole dominion.

If I am asked, what after all is this malaria with which I have been so bewildering myself? I can only again reply—*Nescio*; but I believe I can prove that solar heat is the prime agent of its production—atmospheric stagnation the condition—and the absorption, not the decomposition (for the smell means nothing) of water, the mate-

rial of its existence; for in regard to this last position I have already shown, by reference to the marshes of Guadaloupe, Berbice, and Tobago, that when water is resolved into its constituent gases, these fly far beyond their bed, and leave the more ponderous malaria behind. But I need not have gone so far to seek for proofs, having them at hand in every ship, where the gases arising from the bilge-water never, (and who has not smelled them?) except in very rare instances, prove malarious. Is it then evaporation alone? Certainly not; for wherever evaporation is rifest, as we see in the flooded marsh, it is infallibly extinguished and lost; and that mightiest field of evaporation, the surface of the ocean itself, is the healthiest, and the most devoid of it of any in nature.

Such, to a certain extent, is all visible superficial water. It must first be absorbed and disappear to the eye before it can become mischievous; but after that, whether its clothing be vegetable corrupted remains, by far the most general covering of the earth, and therefore the most suspected; in fact the earth itself; the saturated drying sand of the plains; the drying fen and its spongy bed; or the fissures and crevices of the leeward rock, or any thing else, it matters not; the product will still be malaria, in a greater or less degree:—but a truce to speculation; let us descend to proofs.

During the autumn of 1815 a guard-house, situated at the confluence of the outer and inner ditch of Fort Matilda Basseterre, Guadaloupe, became so malarious that every white soldier, without exception, was seized with fever after passing a single night in it. The guard was therefore committed to black troops, who took it with perfect safety. The state of the ditch became an object of inquiry, which I examined with the utmost care, and never saw a cleaner, apparently a healthier; and the houses of the inhabitants, which had most improperly been built close upon the ditch, felt nothing of the sickness; no more did our general hospital, within short pistol shot. A tiny rill of pure water trickled from one of the rocky sides and slightly moistened the hard bottom; but this could not have been the sole cause, for I afterwards found that every house of any note in Antigua, where there are no springs, had a tank or water-cellar under the dwelling, supplied by spoutings to catch the rain water from the roof; that an officer's quarter of Berkshire Hill, St. Vincent's, was built immediately over one of the garrison water-tanks, yet had always been healthy; and a block-house in Demerara, similarly situated, and full of soldiers, was healthier than the other quarters; to say nothing of the ditches of Fort Edward, Martinique, and others perfectly dry, that were every bit as dangerous; in fact, water in bulk and visible to the eye, while so contained that it cannot be absorbed, I believe, even though most putrid, never to be pestiferous; but whenever it disappears into an absorbent surface, the ground becomes more or less pestilential.

For examples of sandy plains proving malarious I may refer to the southern shores of the Tagus in Portugal, so well known to many during the last war; indeed to the whole province of Alentejo; to the

sandy encampments of South Holland, which our troops occupied in the autumn of 1794, and where I first saw endemic fever spread through an army; to the deep and deadly sands of Grenville Bay,* in the Island of Grenada; to Berbice, which is as sandy as the dried bed of the sea—not an alluvial swamp like Demerara; and lastly, to the provinces of South Carolina, Florida, and all that unhealthiest section of North America, which I believe (for I have never been there) to be essentially sandy.

But none of these can prove unhealthy until they have been first saturated with water and dried on the surface, and it was from them that the best physician our army ever produced (Sir John Pringle) has stated that on arriving at any quarter in Flanders he could at once pronounce upon the health of the place by looking into the wells and seeing how near the water rose to the surface.

From all this the military officer may gather, that although the eye will often deceive, and the nose give only false warning, all epidemic (spreading) fevers, in hot climates, must be malarious, and that malaria being strictly a local product, not unfrequently of very limited range, whenever any encampment, or cantonment, or parts of these, prove unhealthy, to change the ground, even by the shortest move: and to keep changing as long as sickness continues, will often make all the difference between health and disease: move and keep moving while the fever lasts, but always to the windward, never to the sheltered side; and whoever wishes to have a fever let him seek the screen of a rock, or of a hill that rises like a wall, for it is there precisely where the poison nestles. In such situations there can in fact be no cover against it. This is worthy of proof, and I shall here adduce it.

Port of Spain, Trinidad, is situated very near the great eastern marsh, and is therefore far from being a healthy town, but by no means uninhabitable. On the right are some covering heights, between the town and the marsh, which, unlike the site of the town, that has been built on alluvial ground, are composed of the driest and most healthy materials, pure limestone (it may almost be called marble), the purest and the best in all the West Indies; yet have they proved a residence deadly and destructive in the greatest degree to all who ventured to inhabit any part of their diversified surface. No place, however elevated, or sunk or sheltered, or walled in, gives security against the exhalations from the other side. On the highest top, at an elevation of 400 feet, and possessing a fine temperature, a large Martello tower was built to defend the town, but it was found to be as uninhabitable as any of the attempted residences on the near side of the hill below it; and yet, despite of such warnings, did the barrack or engineer department of those days proceed to build the

* Grenville Bay, with its village, is a roadstead, or harbour for export, on the windward side of the island, for that part of the country; and in my report, dated August 1816, I find the following note:—"Even our unthinking, most incautious sailors, dread to sleep on shore there, and the clerks that are employed after crop time to ship the sugars at the harbour are allowed double pay."

barracks of Orange Grove,* at the base of the ridge, for the reception of the white garrison (black troops were then unknown) of the town. They might as well have dug graves for them at once; but such has generally been the case with the old military establishments of the colonies, where the convenience of the engineer and the profit of the contractor seem always to have been the authorized objects, and not the preservation of the troops.

The beautiful post of Prince Rupert, in Dominique, is a peninsula, which comprehends two hills joined to the mainland by a flat square isthmus of about three quarters of a mile in extent, and of the deepest swamp.

The two hills jut right out into the sea, by which they are on three sides encompassed, and between them runs a narrow clean valley, where all the establishments of the garrison are placed. The inner hill, of a slender pyramidal form, rises from a narrow base, nearly perpendicular, above and across the marsh from sea to sea, to the height of 400 feet, so as completely to shut it out from the Post. Trusting to the eye alone, a more complete defence and cover could not be conceived. It was speedily found that the barracks in the valley were very unhealthy, and to remedy this fault, advantage was taken of a recess or platform near the top of the inner hill to construct a barracks, which was thoroughly concealed by the crest of the hill from the view of the marsh on the outside, and at least 300 feet above it; but it proved pestiferous beyond belief, and infinitely more dangerous than the quarters in the valley, within half musket shot below.

Here, in both these instances, we see the inefficiency of shelter, and can only be led to believe that the malaria, through its attraction to the ground, creeps upward, and when it has ascended to the top, through the same attraction drops or dips upon the localities below.

But of all European countries, malarious Spain, with its wide depopulated plains and unreclaimed jungly hills, furnishes the most abundant examples of this poison in all its varieties, for during the latter part of summer and all the autumn it would be difficult to find in any part of the world a more unhealthy country; and it has been so ever since the days of Julius Cæsar, "who had a fever when he was in Spain." The poet does not exactly tell us when, but we may presume that it was in autumn, for it is then, when the streams cease to be streams, and are no more than detached pools in the lines that had formerly been running waters, when vegetation has disappeared from the surface, and ditches, pools, and marshes are all dried up, that pestilence streams forth to the paralyzation of armies. Spain beyond all doubt, though as prolific of endemic fever as Walcheren, is then the driest country of Europe, and it is only when she has been

* Such is the difference between the black and white troops, that in the year 1816 I found a negro regiment living at these barracks in a state of the greatest health and comfort.

thoroughly wetted by the periodical rains that she can be called healthy, or even habitable, with any degree of safety.

Such is malaria. Man alone can war against it, by cultivating the soil and building cities, the interior of which, more especially if they be large, is always proof against it from without; and for this reason it is that all great towns, until they begin to decay, have been found healthy. This is the case, with few exceptions.* Let him halt in the work, and presently the enemy from without is upon him, pressing him back from stage to stage, and re-occupying the ground, until at last he is driven from the land. Read the history of ancient Rome, and compare her greatness then with her present state: the mightiest metropolis the world ever saw—her very size securing health within, and population, by reciprocating cultivation, ensuring the same result beyond the walls; now see her and her territories, with the population, going or gone, fast becoming one vast field of malaria. Or view, on the other side, our own Calcutta, rising, almost within the memory of man, from an obscure fishing hamlet amidst the swamps of the Ganges, into a mighty city, building out the malaria, and sending before her as she expands the pioneer of cultivation into the neighbouring jungles: but let her once recede, or even halt in the course, and the occult enemy from without will be upon her—her palaces will presently become his abode—the worse from being ruinous—and the powers of pestilence, driving her inhabitants from street to street, will at last cause her to sink into the same malarious den from whence she arose.†

Cultivation always and necessarily implying population, is nearly all-powerful against malaria; of which, again, in the western hemisphere we have another beautiful example at Demerara, where the seaward border of the swamp, I may say, of 1000 miles (for it is deep, vast, and immeasurable) has been converted into a habitable and beautiful settlement, as rich as any in the world. A Dutchman alone could have ventured to begin such a work; and through the negro's hand he has actually accomplished it.

But it is now full time to sum up and dismiss the subject. Malaria is universally diffused throughout all hot countries wherever ventilation is wanting or water dried up. It is powerful only during the night, and is certainly dissipated, or, at least, rendered innoxious by the light and heat of the sun in as far as any one given day is concerned. Its tenacity of place, however, is one of its most remarkable

* The health of great towns is regulated by moral, not physical influences, of which last they are in a great degree independent. The principal exceptions that occur to me of this are the great towns of North America and Southern Spain, in certain unhealthy seasons generating yellow fever, and those in the East generating the plague; but even these last may be classed under the above general rule, as it is their defective police that favours the spread of pestilence; for the European factories, where due precautions are always observed, rarely suffer.

† Such, too, will be the fate—the speedy fate—of the beautiful West India town, Port of Spain, whenever she ceases to build and to cultivate.

features, for unless the malarious field be large its range is singularly limited, being often confined to one side of a street, to one story of a house, to one section of a ship, and above all, to umbrageous trees, from which it is so difficult to disengage it, that the inhabitants of Dutch Guiana have ventured to construct their habitations, and that with impunity, provided they have this security, immediately to leeward of the most pestiferous swamps.

We may fairly presume that it is heavier than the ordinary air of the atmosphere, for it creeps along the ground, and is singularly concentrated and attracted by the higher grounds immediately above the drying swamps where it is generated. It lodges in the angles and ditches of fortified lines, and lurks in the nooks and crannies of walled towns; but the pavements of these last are the best defence against it, and an unbroken extent and succession of these confer as nearly as possible perfect safety; for all which, if there be a remedy, it must be found in the powers of cultivation ever opening the surface for the escape of pestilential gases, and exhausting the morbid principle by a constant succession of crops; for wherever malaria prevails, the uncultivated savannah, even though used for pasture, becomes more unhealthy than the plantation, and the depopulated country falls completely under its dominion. These, in as far as I know, are its principal attributes; and if I have been successful in pointing them out so as to be intelligible to the military reader, future encampments may derive the benefit, and military life be better preserved.

To the proposal of your correspondent for encamping, that is tenting, our white soldiers in windward situations during the hurricane or unhealthy season, I must, with all due respect, enter my hearty protest against its ever being attempted in any quarter of the West Indies, with the exception of those highly elevated ones, when the temperature being the same as that of a European climate, it could be done with equal safety. In the lower levels I have seen the afternoon heat in the single unlined tent (and our Service admits of no other), even when inhabited by an individual officer, rise as high as 110° , 112° , and 114° of Farenheit, and sink nearly 50° under the chilling dews of the early dawn. No human constitution could endure such vicissitudes long, and a row of tents would literally become a plantation of dysentery.

The worst house has been said to be better than the best tent; yet hutting, in the case of our white troops, is nearly as objectionable. The hut being a far better protection against the alternatives of heat and cold, would be less productive of dysentery, but far more prolific of fever; for ground emanations, of which malaria is the chief, have always proved pernicious to them—so much so, that there has constantly been a marked difference between the health of those inhabiting the lower and upper story of the same barrack, even when the building was well raised off the ground, the former often giving as many as one-third more sick. The black, as I have stated in a

former letter, seldom or never suffers from malaria, and to him the hut is congenial in every respect.

One use I conceive can be made of huts, which may be worth mentioning here, and that is to hut a body of white soldiers on any ground that may be proposed for the site of barrack buildings. It would be the *experimentum crucis*. Let them so occupy it for a few weeks during the hurricane or unhealthy season, and if in that time no epidemic (spreading) fever broke out amongst them, we may be assured the ground is safe. *Experimentum in corpore vile* is an old adage; but to hut the black would prove nothing, for he is fever proof; and, besides, we could always remove the white soldiers as soon as the existence of malarious fever was ascertained.

I write all this as being applicable only to the lower levels of a tropical country. The higher, when they can be attained, present the climate of Paradise—a perpetual summer, without the vicissitudes of season: so much does the coolness of elevation surpass that of latitude; or, if the sojourner so please, he may ascend to that of spring or winter, and there seek the reinvigoration of his relaxed frame.

The recommendation of your correspondent for supplying wire-gauze blinds to barracks in malarious quarters seems to be an excellent one, and deserving of every consideration; for if, as we have seen in the safety lamp, the most subtle of gases, the choke-damp of mines, can be so decomposed by it as to be rendered inaccessible to the most subtle and powerful of the elements, we have a right to presume that the more ponderous malaria would still more easily be made subject to its powers.

The expense may be an objection, for the climate of the West Indies soon destroys everything of iron, and copper-wire would be costly. I would advise the experiment being made in the first instance with the hardest spun cotton-gauze. The veil has proved effective against the deadly malaria of the Pontine Marshes: but here I must guard myself from passing an unqualified opinion, for we know not whether these veiled travellers allowed themselves to sleep during the transit, and it is my belief that malaria can only prevail upon the body during the passive state of sleep; in fact, that to sleep is the danger.

Experience and observation have seemed to me to confirm this belief; and I certainly would not hesitate to warrant the safety of any traveller crossing at night the most deadly jungles of the East, provided the journey were performed awake and alert on horseback—not quiescent in the sleepy palanquin; and then, I confess, I would prefer a cigar in his mouth, and a brandy-flask in his side-pocket to a veil over his face.

I have now done with malaria, for it would be unfair to trespass farther on your pages. It was my intention to have appended a note upon the supposed contagion of yellow fever and contagion generally; not a technical disquisition, intelligible only to the professional reader, but such a plain view of the question as may give confidence and know-

ledge to every one liable to serve in tropical climates. I shall still send it for your consideration, along with another letter on the barrack hygiene, if I may use such a word, of the soldier in the West Indies.—*United Service Journal*, Nov. 1837.

Transfusion of Blood.—Prevost and Dumas showed that the vivifying power of the blood does not reside so much in the serum as in the red particles. An animal bled to syncope, is not revived by the injection of water or pure serum of a temperature of 68° Fahr. into its vessels. But if blood of one of the same species is used, the animal seems to acquire fresh life at every stroke of the piston, and is at last restored. Professor Dieffenbach has confirmed these experiments. It is stated by Prevost and Dumas, and by Dieffenbach, that revival takes place likewise when the blood injected has been previously deprived of its fibrin. I have shown that the red particles of the blood remain perfectly unchanged after the removal of the fibrin; blood, therefore, from which the fibrin has been removed, and heated to the proper temperature, ought to be preferred in the few cases where transfusion of blood is justifiable, or necessary, on account of hemorrhage; for in this state the blood is completely fluid and remains so, and thus the principal difficulty of transfusion, namely, the ready coagulation of the blood in passing from one animal to the other, is avoided. Blood of animals of a different genus, of which the corpuscles, though of the same form, have a different size, effect an imperfect restoration, and the animal generally dies in six days. The pulse becomes quicker, the breathing remains natural, but the temperature sinks very rapidly; the excretions are mucous and bloody; the cerebral functions seem to be unaffected. The same symptoms ensue when the serum and red particles without the fibrin are injected.

The injection of blood with circular corpuscles into the vessels of a bird (in which the corpuscles are elliptic and of larger size), produces violent symptoms similar to those of the strongest poisons, and generally death, which ensues, indeed, instantaneously, even when a small quantity only of the blood has been injected; such, for example, was the effect of the transfusion of some blood of the sheep into the veins of a duck; while in many cases in which the blood of sheep and oxen was injected into the vessels of cats and rabbits, these animals were revived for a few days. The fact of the blood of mammalia being poisonous to birds is very remarkable; it cannot be explained mechanically. The injection of fluids containing globules of greater diameter than the capillary vessels produces death by obstructing the pulmonary vessels, and producing asphyxia; but the corpuscles of the blood in mammalia are even smaller than those of birds. In Dieffenbach's* numerous experiments, pigeons were killed by a few

* Die Transfusion des Blutes von Dieffenbach. Berlin, 1822.
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drops only of the blood of mammalia. The blood of fishes is said to be fatal to mammalia as well as to birds.

[The interesting experiments of Dr. Bischoff† throw new light on the subject of transfusion. He confirms the statements of Prevost and Dumas, and of Dieffenbach, as to the deadly effect of the blood of mammalia injected into the veins of birds. In all his experiments made with the fresh blood of mammalia, the birds (common fowls) died within a few seconds after the performance of the transfusion, with violent symptoms resembling those of poisoning. But when, instead of the fresh unchanged blood, he injected blood from which the fibrin had been removed by stirring, and which was heated to the proper temperature, he was surprised to find that no symptoms were produced,—the animal appeared to suffer no inconvenience. These experiments were performed repeatedly, so that there could be no fallacy in the result. I was present when Dr. Bischoff performed them before his class at Heidelberg in July, 1835. The deadly effect then of the blood of mammalia on birds is in some way connected with the fibrin of the blood. The principle which renders the blood of one class of animals thus injurious for another class, is not, Dr. Bischoff remarks, identical with the vivifying principle of the blood, which might be supposed to be peculiar to each individual class, and deadly to others; for the blood, when thus deprived of its fibrin, has still the effect of perfectly restoring the animal from which it was taken, although the latter be reduced by loss of blood to extreme syncope or apparent death: but it is an important fact, that when blood thus deprived of its fibrin is injected into the veins of an animal of a different class, reduced to a similar state of syncope, no revival takes place,—the animal dies. Hence the blood of an animal of a different class, even when deprived of its fibrin, although not poisonous, is not adapted for the operation of transfusion, in cases where this is necessary in man.

Dr. Bischoff mentions but one experiment in which he injected the blood of a hen (about half an ounce), deprived of its fibrin and warmed, into the vessels of a dog, and in this instance no other effect was produced on the animal than a state of exhaustion which might be the result of his struggles during the operation.

The experiments of Dr. Bischoff on the transfusion of different kinds of blood into the veins of frogs, are, from the difficulty of the operation, less satisfactory. The results, however, which he has deduced from them seem to be tolerably certain. The blood he used was in all cases deprived of its fibrin, and its effects so far corresponded with those on the higher classes of vertebrata that it did not produce an immediately fatal result; but it nevertheless had a marked injurious effect on the system, and this was most violent when human blood was injected, less so when that of mammalia and birds was used. The blood of fishes had in several instances no particular

* Møller's Archiv. 1835.

effect. When the blood of crabs was injected, the frogs lived several days, but died eventually. The effect of the transfusion of the blood of man, mammalia, and birds, was always death, generally in a few hours, the only symptom being diminished activity of the circulating organs; the heart in some cases seemed to be paralysed. After death there was found in almost all cases effusions of a reddish serum, containing the red particles of the frog mixed with those of the blood injected, particularly in the stomach and abdominal cavity.]—*Muller's Elements of Physiology*.

NOTICES OF WORKS RECEIVED.

“*Statistics of Fever and Small-pox in Glasgow*. By ROBERT COWAN, M.D., one of the Physicians to the Glasgow Royal Infirmary, 1837.”—In this paper, which was read before the Glasgow Statistical Society, Dr. Cowan investigates three important subjects:—

I. The Statistics of Fever in Glasgow for the last forty-two years.

II. The Statistics of the Glasgow Fever Hospital for the year ending November, 1836, and

III. The Statistics of Small-pox in Glasgow.

Dr. Cowan commences with giving summaries of the weather in Glasgow. These we shall pass by, as we believe that no accurate results have as yet been obtained with respect to the relation of atmospheric phenomena to disease.

The population of Glasgow has been of late increasing with extraordinary rapidity. In the year 1829, the numbers were 147,197. In 1821, the number of the population was a little less; but in 1831, it rose to 202,426; giving an increase in ten years of more than 55,000. This increase Dr. Cowan attributes, in a great degree, to immigration. The great demand for females alone, in the vast cotton factories of Glasgow, explains the interesting fact, that a large portion of the immigrants were females from 15 to 25 years of age; a fact of some importance with respect to the prevalence of fever, as we shall hereafter see.

“In 1819, there was one Irish person out of every $9\frac{1}{100}$ of the inhabitants, and in 1831, one out of every $5\frac{99}{100}$.”* From this increase of Irish alone, without including the influx of labourers from the Highlands and Lowlands of Scotland, it is quite obvious that the relative proportion of the middle and wealthier classes to the labouring class must have been yearly diminishing; and, hence, one source of the increasing rate of mortality in Glasgow.”

The mean mortality of the city, from 1821 to 1835, as deduced from the researches of Mr. Paul, whose labours on this subject are above praise, and whose mortality bills should serve as models for all future investigations of this kind, is 1 in 33.24. In 1836, the mortality was so great as 1 in 26 and a fraction. This great increase is in part attributed by Dr. Cowan to the prevalence of fever. The returns

* Dr. Cleland.

of the Fever Hospital shew that in 1835, the admissions only amounted to 1359, and on an average of the last eight years, to 1477, while in 1836, the number was 3125.

One of the most remarkable results of Dr. Cowan's investigations is the demonstration of the singular increase of fever in Glasgow. He divides the period from the year 1795 to 1836, into four septennial periods.

"In the first septennial period, the fever patients treated in the Infirmary were— 12.92 per cent. of the whole.

In the second,	9.84	—	—
In the third,	8.17	—	—
In the fourth,	31.77	—	—
In the fifth,	36.19	—	—
In the sixth,	49.96	—	—

and if to this table, strictly applicable to the Royal Infirmary, we add the numbers treated in the temporary hospitals, we will raise the per centage in the fourth period

From 31.77 to 47.62 ; and in the sixth period,
From 49.96 to 54.83.

During the first 35 years embraced in the table, the number of patients affected with fever treated in the infirmary, amounts to 11,511, while in the last seven years it amounts to 11,751."

The following observations are of great importance :—

"Causes, peculiar to Glasgow, giving rise to fever, and favourable to its propagation, must exist, and it is the duty of our civic authorities to investigate these causes.

"Manchester, with a population, at the last census, of 227,808, and which, in its constitution and density, must nearly resemble that of Glasgow, has been for years, and is now comparatively free from fever. The average annual number treated in the Manchester Fever Hospital for seven years, ending in 1836, was 497

The annual average in Glasgow during the same period, 1842

The number treated in Manchester Hospital in 1836, 780

The Glasgow, 3125

Fever is now diminishing in Manchester, while it is increasing in Glasgow.

"The prevalence of fever in Glasgow, when compared with Manchester, is still more strikingly contrasted by the great change which has taken place in this respect. From 1797 to 1806, both inclusive, the number of the fever patients treated in the Glasgow Infirmary was only 883, while those treated in the Manchester Fever Hospital amounted to 4618.

"In Leeds, too, another manufacturing city, with a population at the last census of 123,393, the number of patients affected with fever, and treated in hospital, amounts to 1923 during the last seven years, giving an annual average of only 274.

"In Newcastle and Gateshead, with a population of 57,917, the number of patients treated in the institution for the cure and prevention of contagious fever during the last seven years, amounts to 276, or 39 annually.

"In Liverpool, with a population of 189,242, 1700 cases of fever were treated in the hospital during 1836 ; but many of these belonged to the seamen of the port, a numerous class of its population.

"A comparative view of the state of fever in other towns in England, contrasted with that of Glasgow, would, I am afraid, only place the insalubrity of our city, as far as fever is concerned, in a more prominent and alarming point of view."

After some temperate but excellent strictures on the general want of means to meet the pressure of an epidemic fever, Dr. Cowan continues—

“ We have proved that since 1816, but more particularly during the last seven years, fever has been steadily increasing in the city of Glasgow, and that its victims constitute within a fraction of 55 out of every 100 patients treated in our hospitals, independently of those treated by the district surgeons within the burgh.

“ *This increase, especially during the last seven years, has taken place, not in years of famine or distress, but during a period of unexampled prosperity—a period when the wages of labour have been ample—the prices of provisions comparatively low, and every individual, able and willing to work, secure of steady and remunerating employment.*

“ True, indeed, the weather has not been favourable since 1830 ; and certain atmospheric phenomena inimical to health have existed, as may be inferred from the prevalence of various epidemics, though not appreciable by scientific instruments ; but these general causes have not acted so severely on other cities of the empire as they have upon Glasgow, which has numbered more victims from influenza, cholera, and fever, in proportion to its population, than any other city in Britain.

“ Many of the causes of the production and propagation of fever must be ascribed to the habits of our population ; to the total want of cleanliness among the lower orders of the community ; to the absence of ventilation in the more densely peopled districts ; and to the accumulation, for weeks or months together, of filth of every description in our public and private dunghills ; to the overcrowded state of the lodging-houses resorted to by the lowest classes ; and to many other circumstances unnecessary to mention.”

We now proceed to the Statistics of the Glasgow Fever Hospital, from the year ending on the first day of November, 1836. This hospital can easily accommodate 220 patients ; and during the year above mentioned, 2,655 patients were admitted, of which 2,513 were treated by Dr. Cowan himself.

“ The average residence of each patient in the hospital was 18 days. Of the 2513 patients under my charge, there were—

	<i>Scotch.</i>	<i>English.</i>	<i>Irish.</i>	<i>Total.</i>
Males, . .	818	37	400	1255
Females, . .	885	16	357	1258
	<hr/> 1703	<hr/> 53*	<hr/> 757	<hr/> 2513

“ The males and females were nearly equal in number, being 1255 and 1258. The Scotch were 67.76 per cent. of the total admissions.

The Irish, . . 30.12

The English, &c. 2.10

“ Although the fever hospital is strictly appropriated to the reception of patients labouring under fever, small-pox, scarlet fever, measles, and erysipelas, still patients affected with other ailments are occasionally sent there, either from their diseases being mistaken for fever, or from the facilities of admission being greater than those of the infirmary.”

We next come to an investigation which is to us peculiarly interesting, it relates to the proportions of Scotch, English, and Irish treated in the Hospital. Dr. Cowan says :

* Including 13 foreigners and those born in the colonies of Great Britain.

" Of the fever patients, the Scotch form 66.10 per cent.

English,	2.12	...
Irish,	31.67	...

" Of 95 patients with small-pox, 91 were Scotch and 4 Irish, and all were, with two or three exceptions, above 20 years of age,

" Of 61 patients with scarlet fever, 50 were Scotch, 10 Irish, and 1 English.

" The two following tables exhibit the number of males and females, according to the government census of 1831, and also the number of Scotch, Irish, English, and foreigners at the same time.

		I.			
Males.		Females.		Total.	
93,724	108,702	202,426	
II.					
Scotch.	English.	Irish.	Foreigners.	Total.	
163,600	. . 2,919 . .	35,554 . .	353 . .	202,426	

" The proportion of Irish treated in the Fever Hospital is much less than what is generally believed by those who have not paid attention to the subject. Dr. Lombard, of Geneva, estimates the number of Irish resident in Glasgow at 60,000, and ascribes the prevalence, and what he deems the peculiarities of our fever, to the number of Irish resident in Glasgow.* The author of the article ' Vital Statistics,' in M'Culloch's Statistics of the British Empire, vol. ii. p. 572, makes the following remarks:—" The increasing mortality in Glasgow is no doubt in part due to the accession of Irish population, who amounted, in 1831, to more than 1-6th of the inhabitants. The poor Irish, we strongly suspect, are keeping up, if they be not introducing, the fevers of their wretched country in the heart of the British cities. This is confirmed in the case of Glasgow, by the ages at which the mortality is augmented, and by a report of the Glasgow Infirmary before us, from which it appears that, in the year 1835, out of 3260 patients treated, 1258 had fevers, and of these 125 died."

" This statement will be proved to be incorrect while adverting, at the close of this essay, to the influence which fever has had in augmenting the mortality of Glasgow, especially during 1825, the year alluded to in the quotation."

It is scarcely worth while to observe upon the opinions of the author of the article *Vital Statistics* in M'Culloch's Statistics of the British Empire. It is difficult to say whether his ignorance exceeds his insolence; and we would class him with those deep thinking and humane political economists, whose representative in the Edinburgh Review wrote, that it would be well for England if the skates and codfish swam over the fair fields of Ulster. There is not the slightest evidence that the labouring classes of Irish introduce fever into the "heart of the British cities." Dr. Cowan shews that the statement of the writer above alluded to is wholly incorrect, and that the increased mortality of Glasgow must be attributed to other causes besides fever; of these small-pox is a most important one. By a careful investigation he has shewn that a very large proportion of the mortality has occurred under ten years of age, a fact sadly at variance with the Anti-Irish staticians above quoted. In the years 1835 and 1836, the deaths from fever were 1253; and the deaths from small-pox 1050. *Now of the deaths from fever, 186 only were*

* Dublin Medical Journal for September 1836.

of individuals under ten years ; while of those from small-pox, 993, or nearly the whole, were under this period.

Dr. Cowan refers also to the opinions of Dr. Lombard of Geneva, which are given in two papers in this Journal, for September and November, 1836.—For the information of our readers we may state, that Dr. Lombard has taken up the idea, that among the obligations which Great Britain owes to her humble sister, is to be enumerated the introduction of fever of a peculiar kind,—the receipt for the manufacture of which has been lost in England and on the continent, but has been carefully preserved by the Irish, and handed down from generation to generation. This fever he denominates the Irish typhus, and maintains that it is characterized by a predominance of cephalic symptoms, by being highly contagious, and by its *not presenting abdominal lesions*, an anatomical character of such importance, that we regret extremely that Dr. Lombard has not produced more than three cases in support of his very extensive proposition. Had Dr. Lombard made more inquiries, he would have found, that Ireland is not so sunk in misery and debasement, but that she can produce occasionally, a fever which, in abdominal ulcerations, can compete with the sporadic diseases of her wealthier and more enlightened neighbours : with us the *dura ilia messorum* are not always so intractable. We have epidemics with and without a predominance of gastro-intestinal disease, and the absence or presence of the follicular ulcerations, cannot be made a ground of distinction between the fevers of Ireland and those of other countries. It would be well if Dr. Lombard had remained content with his first paper, it is written in a good spirit, and is chiefly suggestive ; but when, in his second, he permits himself to be swayed by the vulgar system of abuse of Ireland, and in the face of his own facts, (which shew that in the year 1836 that Manchester, Birmingham, and London, cities having a vast Irish population, had an extremely small number of fever patients,) declares that wherever the Irish journeymen go, they carry with them the contagion of fever, we would fail in our duty as journalists, did we not express our entire dissent from the proposition. His theory of Irish fever is anatomically incorrect, and if we wanted facts in addition to those given by Dr. Cowan and by Dr. Lombard himself, we might refer to the annual and vast immigrations of the lowest order of Irish into England, for the purpose of saving the harvest, which, were Dr. Lombard's theory true, should be followed by corresponding epidemics of typhous fever. The author next proceeds to give some important tables constructed with reference to the age, sex, and mortality of the patients. We regret that our limits will not permit us to give these tables, but we may here quote Dr. Cowan's general conclusions.

“ The first point that attracts our attention is the relative mortality of the two sexes, and certainly it is very remarkable. The total mortality of the males is 1 in every 61 $\frac{2}{3}$, while of the females it is only 1 in every 11 $\frac{2}{10}$.

“ In the males the mortality is 14.83 per cent.

“ In the females 8.92 ..

“ The deaths of the males within the first 24 hours amount to 17.

" The deaths of the females within the first 24 hours amount to 9.

" At almost every period of life embraced in the Table, the mortality of the males from fever exceeds that of the females.

" At the age of 15 the mortality is very nearly the same in both sexes.

" At the age of 30 the mortality of the males is more than double that of the females.

" The rate of mortality is greatest in females at the age of 45.

" The mortality of the males under 20 years of age, 6.04 per cent.

.. .. females 4.90 ..

" The total mortality under 30 years of age, 8.35 per cent.

.. .. above 30 24.84 ..

" From the table of mortality without reference to sex, and which is a combination of the first two tables, it appears that, after the age of 10, the mortality from fever slowly increases till the age of 35. From the mortality being 2.63 per cent. at 10 years of age, it has gradually risen to 14.92 at 35; at 40 it is 22.36 and at 50, 39.06.

" The mortality of the Scotch and Irish was precisely the same, while that among the English, if any inference can be drawn from such a small number, was considerably less."

The petechial eruption in fever has of late attracted much attention, from the analogy which seems to exist between it and the exanthemata. Two forms of petechial spots must be admitted to occur in fever. The one connected more or less with debility of the capillaries, and a dissolved state of blood, and analogous to the spots in purpura, while the other is an active rather than a passive local condition, and exhibits many of the characters of an inflammatory cutaneous efflorescence, appearing at an early period, exhibiting a distinct elevation of the cuticle, and with a vascularity which can be made to disappear under pressure. In addition to these characters, the petechial eruption has been observed to be more or less critical, to be followed by relief of internal congestions or inflammations; to appear first on the upper portions of the body, and to shew some phenomena of periodicity. Dr. Cowan has examined the question, as to how far the eruption is to be considered characteristic of typhus. He says—

" From these tables it appears that the proportion with eruption varied each month; and that, with the exception of the quarter ending in October, the number of females with eruption was always less than that of the males.

" For the first six months of my attendance, less than one-half of the females had eruption, while in the last six months four-fifths had it; and, upon the average of the whole year, 71 out of every 100.

" Of the males the monthly proportion also varied considerably, but on an average of the first and second quarters, 63 per cent. had the typhoid eruption.

" During the last six months the proportion of males and females in which the typhoid eruption appeared was nearly the same. At the close of the year, in 76.16 per cent. of the males, and 71.77 of the females, the typhoid eruption had occurred, giving as an average of the whole cases 73.99 out of every 100 admitted.

" From these tables I am warranted in the inference, that the exanthematous eruption is not an essential character of the fever of this country, as during the first six months it occurred in only 49 per cent. of the females, and 63 per cent. of the males; and, besides this, even in an epidemic, in which it is a distinguishing

feature, it is not invariably present, as during the last six months it was absent in nearly one-fifth of those admitted."

On this subject we would refer to a paper by Dr. Perry, of Glasgow, published in the 10th volume of this Journal; and also to the printed thesis of our friend Dr. Julius Staberoh, of Berlin—a gentleman whose visit to Dublin will long be remembered with pleasure by all who had the happiness of his acquaintance.*

We now come to Dr. Cowan's researches with respect to small-pox in Glasgow. They are of such importance, as shewing the value of *vaccination*, that we shall scarcely abridge them.

"The introduction of inoculation, although it diminished the relative mortality, will, it is believed, be found to have increased the absolute mortality of small-pox; as by this practice the disease which, before its introduction, occurred epidemically, and only, at long and uncertain intervals, was kept constantly prevailing at all times and seasons, thereby producing a mortality, especially among children, which could now be scarcely credited, but for the attested registers of its ravages. The fact is undoubted, that small-pox inoculation did not effect that saving in human life so generally attributed to it. While it was adopted by the upper and intelligent classes of the community, it was rejected by the lower; and the bills of mortality prove the deaths by small-pox to have increased, after the practice of inoculation was introduced. In this city small-pox inoculation was generally practised, and recommended by medical practitioners, during the period embraced in the following table. We have no data from the Glasgow bills of mortality to prove the fact of the mortality from small-pox being greater at the close than at the commencement of the eighteenth century, but it has been ascertained in other towns, and the following extract from Heberden, confirms the assertion in regard to London. 'Out of every thousand deaths in the bills of mortality, the number attributed to the small-pox, during the first thirty years of the 18th century, before inoculation could yet have had any effect upon them, amounted to seventy-four. During an equal number of years, at the end of the century, they amounted to ninety-five. So that, as far as we are enabled to judge from hence, they would appear to have increased in a proportion of about five to four.' I have, therefore, no hesitation in ascribing a large proportion of the mortality recorded in the following table to the practice of inoculation for the small-pox, being borne out in my assertion by the above quotation from Heberden, and by the medical statistics of other cities."

After giving a table of the total deaths under ten years of age, and the deaths under ten from small-pox in Glasgow for thirty years, and divided into three equal periods, Dr. Cowan observes—

"The ravages of small-pox were never before more vividly illustrated than in the foregoing table.

"In the first period of ten years, the total deaths under ten years of age amounted to 9919, and the deaths from small-pox to 3466, being 35.94 per cent., and rather more than one-third of the whole deaths under ten.

"In the second period, the total deaths under ten are 9080, and the deaths from small-pox 2894, or 31.87 per cent.; and in the last period, the total deaths under ten are 20,913, and the deaths from small-pox 1013, or only 9.28 per cent.

"The saving of human life in infancy by the introduction of vaccination is thus most satisfactorily established, as the table shows an improvement to the ex-

* Dr. Staberoh's thesis is entitled, "*Dissertatio inauguralis medica de Typho Exanthematico Halis epidemico, Berolinensis, 1834.*"

tent of 25 per cent., and if to this be added the lives saved above ten years of age, which we have no means of exhibiting from the Glasgow mortality bills, we will be able to judge of the benefits conferred on society by Jenner.

"I am not aware that small-pox was so fatal in any town as it appears to have been in Glasgow. In Berlin, the deaths from small-pox were, for a short time, as 1 in 4, but more generally as 1 in 7, of the whole deaths under ten years of age, while in the city and suburbs of Glasgow, it was fatal in the proportion of one in three of the deaths under ten years, and that not for one or two years merely, but for a long period.

"The great saving of human life is rendered apparent from the third period embraced in the table. Up to the very moment of small-pox inoculation being superseded by cow-pox the mortality is immense, and the instant the latter is employed, the mortality becomes trifling in comparison."

Out of ninety-five patients treated by Dr. Cowan in 1836, four only were natives of Ireland; while ninety-one were Scotch, of whom seventy were Highlanders, and twenty-one natives of the Lowlands. A very large proportion of the Highlanders were from the remote islands, and all had recently arrived in Glasgow. Fifty-five of the patients had apparently been vaccinated; but forty never had the operation performed. No death occurred in any individual who presented the appearance of having been properly vaccinated.

We would refer to the remarkable fact of the immunity of the Irish from small-pox. Dr. Cowan attributes this, and we think, justly, to the general practice of vaccination among the lower classes by the surgeons of the county infirmaries and dispensaries of Ireland; and the fact must be most gratifying to our distinguished medical friends who have so long presided over our national vaccine institution in Dublin.

It is superfluous to add a word in commendation of the industry, accuracy, temper, and judgment displayed by the author in these researches. We consider his paper as the most important of its kind that has ever issued from the British medical press.

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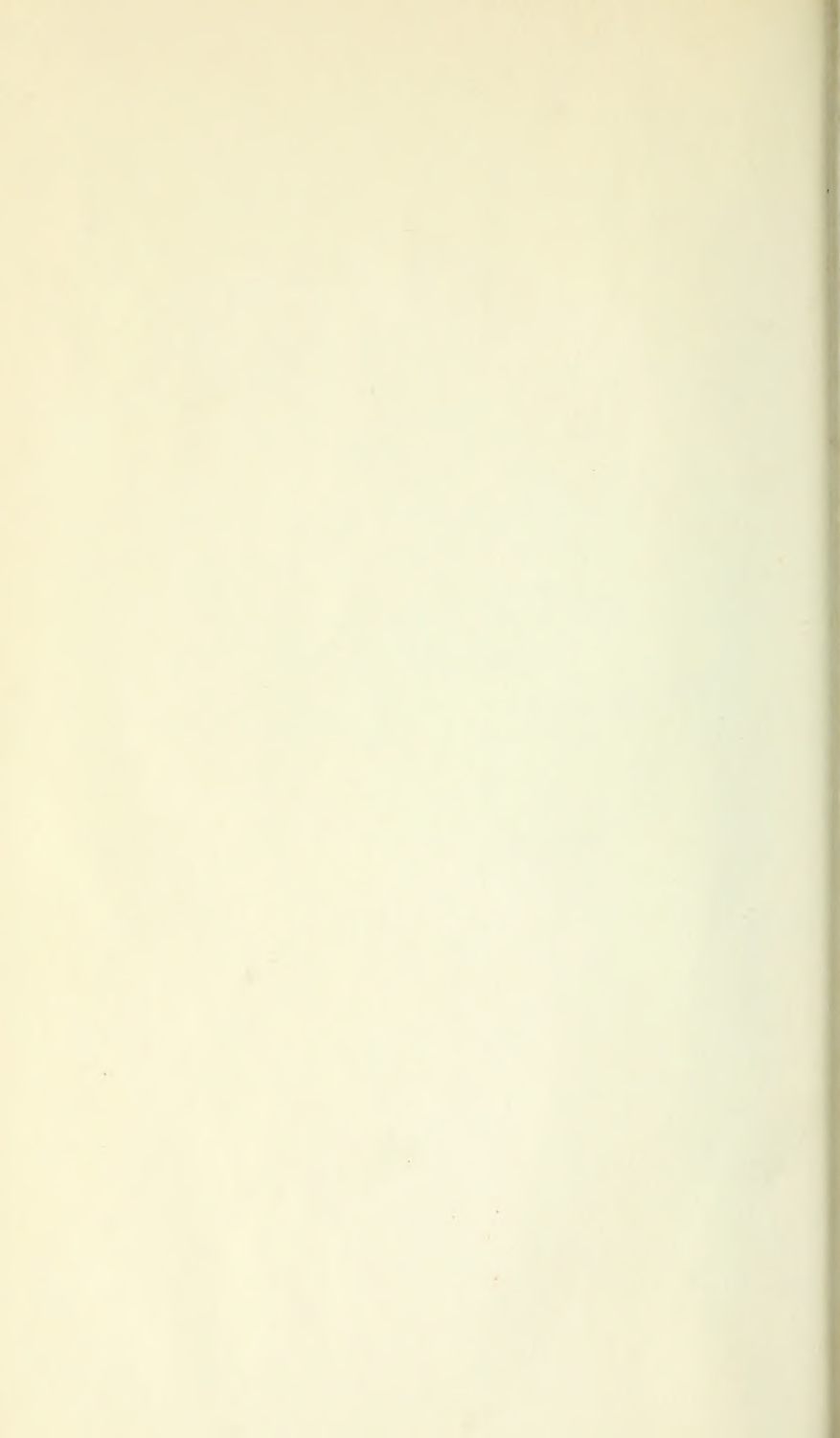
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